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PIC LA RHUNE*

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PIC LA RHUNE, in Basses Pyrénées a few kilometers south of Ascain and west of Sare, is a typical mountain of the lower western Pyrénées. It lies on the border between France and Spain and, although it is only 900 meters in altitude, there is a magnificent view to the west and north of the coast and sea from Hendaye past St. Jean-de-Luz and Biarritz to beyond Bayonne. To the northeast the Col de St. Ignace leads into the Nivelle. In this valley and in the Nive beyond there is spread out a tapestry of small Basque farms and villages with their white stucco walls and rufus tiles. All else is an array of mountains, one behind another in rhythmical patterns until the more distant ones blend with the imagination in a confusion of soft colors, with earth and sky becoming one.

Photos by Dr. M. L. Holt

GI BOTANISTS ON PIC LA RHUNE NEAR BIARRITZ, FRANCE

Our day of botanizing on Pic la Rhune probably is of less interest than the fact that a civilian professor in uniform and a group of soldiers were on a field trip as a class of an American university abroad. It is, therefore, in order to tell a little about Biarritz American University before describing the results of the excursion.

Long before VE-day certain officers of the Information and Education Division of the Army had been making provisional plans for the establishment for soldiers of collegiate-level institutions in Europe as soon as conditions permitted. Within about two weeks after the European victory the program had gone up through channels and had received the green light from General Eisenhower, and one of the most amazing educational experiments of all time was under way. Late in May certain civilian educators who were to play leading roles were called into Washington to help the Army select the faculty and plan the programs for two universities of 4000 students each, one to be located in England and one in France. Among these key men were John Dale Russell, who became Dean of Biarritz American University, and Merle G. Coulter, who helped select the biological personnel for the institutions. Fast

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action was requisite for all aspects of the installation if the schools were to open during the summer. How it went can be suggested by my original contact with the project.

On the morning of the sixth of June, while pulling onions from my garden at Knoxville —we have *Allium vineale* in everything down south—I was told Washington was calling, and went in to a conversation very much as follows:

"Hello, Stan. This is Merle Coulter. How would you like to go to France?"

"What is the deal?"

"The Army is setting up a university at Fontainbleau and we want you to come along. You will get your base pay plus 25 percent for overseas duty, a seven to twelve months' contract, and brass buttons to wear at Courts Martial. How about it if you can get a leave of absence from the University?"

"It sounds interesting. When can I let you know what my wife and Dean think?"

"Call me back this afternoon."

So it started, and so it has been ever since. Rapid decisions, rapid developments, rapid changes. Within thirty hours I was on my way to Boston to be processed by the First Service Command as an "Assimilated Field Grade Officer." I was the first civilian professor to be processed for overseas duty by the Boston Command, and, having no specific directions from Washington, they did everything they could think of, for me and to me. By July fourth about 150 of us from the four corners of the States were assembled at the New York Port of Embarkation for sailing on the *Queen Elizabeth* as shipment IJ B411 LP, the first of the masculine academic WACs.

We landed at Greenock on the Firth of Clyde, and those of us destined for France went down to Southampton by night train, crossed to Le Havre in the hold of a converted freighter, and on to Paris, again by night train. Part of the travel was luxurious, and part was quite the reverse, but when at dawn we could see the Eifel tower in the distance after several almost sleepless nights, we knew we would make it and were glad.

About the time we had left New York it was decided to locate the French school at Biarritz instead of Fontainbleau, and the Station Complement had moved in and set to work preparing the physical facilities for a university in a world-famous resort town where there had never been more than a branch of the Bayonne Lycée. Although that is another story, it would be unnecessarily concise not to mention that the library is located in the former gambling room of the Casino Municipal. The hazards of the roulette table are now replaced by the greater hazards of the printed page. The Physics laboratory is in the rotunda of another casino, Biology and Chemistry are in hotels, Mathematics in a small hospital, and Geology is in a private villa. The students are billeted in the better hotels, including the Palais of Napoleon and Eugénie, and the professors and Army officers are billeted in villas, some in town and some as far out as five kilometers.

When one considers the multitude of details involved in the organization and establishment in so short a time of an installation for 4000 students, 300 faculty, and about 1500 Station Complement, it is little less than amazing that the classes commenced on August twentieth as planned. It is true that at first some laboratories were not yet completed, many classes were without textbooks, some branches were understaffed, and some teachers were without their trunk lockers which contained notes and course outlines as well as spare clothing, but the faculty is composed of experienced teachers and the program went on in spite of handicaps and petty annoyances.

The University is organized in eight sections, or colleges, and forty branches, or departments. About 330 courses are listed in the catalogue and, although most of them are lower division courses, many of them are of Junior and Senior level and some are graduate. The student selection was well made, and the ability, enthusiasm, and appreciation of the soldiers, so recently from battle and the sundry duties of war, are extraordinary. Although the recreational aspects of life at Biarritz are certainly no deterrent to his desire to attend Biarritz American University, almost without exception the student

here really wants an education and earnestly does his best during the intensive eight-weeks' term.

The students on the Pic la Rhune trip are members of my class in Biology 205, Plant Classification. They have previously taken from one to several courses in botany and all are eager to learn more about plants in general and the local flora of the southwest of France in particular. The course is organized on a basis of three hours of lecture or lecture-discussion and three two-hour periods of field or laboratory work a week. In addition, an all-day excursion is taken nearly every Sunday. The trip to Pic la Rhune is, then, simply an example of the work.

We left Biarritz in a weapons carrier Sunday morning the sixteenth of September; travelling southwest along the coast through Bidart and Guéthary to St. Jean-de-Luz we turned eastward up the Nivelle through Ascain to Col de St. Ignace at the foot of the mountain. Our climb followed the route of the funicular railway which has not been in operation recently. Our attention during the first part of the climb was centered on the heathers, with each student trying to find as many types as possible. At the head of the Col, nearly half way up, we stopped to rest and identify our collections. We sat on a steep grassy slope with a rugged Triassic conglomerate hogback behind us, the whole valley below us with its cover of gorse and bracken heath and occasional small stone-walled pastures, and the distant mountains before us.

The handsomest of our heaths was *Daboecia polifolia* Don, with large urceolate lilac to rose-pink corollas and ovate to narrowly oval leaves, shining above, white-pubescent below, and ciliate on the margins. This beautiful undershrub is usually 20 to 30 cm. tall, but occasionally reaches half a meter in favored sites. Besides occurring in a few localities, especially in the southwest of France, it is one of that geographically interesting group of plants that occur in Ireland and the Iberian Peninsula. The needle-leaved genus *Erica* was found to be represented by four species. Of the two species with glabrous sepals, *E. vagans* L. was readily distinguished from *E. cinerea* L. by its exserted anthers. Of the two species with ciliate sepals, *E. Tetralix* L. and *E. ciliate* L., the shapes of the corollas permitted easy separation. The corolla of the former is globular-urceolate and that of the latter is elongate, constricted at the throat, and somewhat arcuate. All of these species existed in color forms ranging from nearly white to lavender, lilac and rose tints. These species are all common in the southwest of France and extend from the Iberian Peninsula, especially Portugal, to the northwest of Europe in the areas of the marine west-coast climate. *E. vagans* and *ciliaris* occur in Ireland; *E. Tetralix* and *cinerea* extend into Scandinavia. The sixth species of heath is *Calluna vulgaris* Salisb. In addition to the ordinary form, it is present on Pic la Rhune in two modifications: var. *condensata* Lemotte, with tightly appressed overlapping leaves in four sharply aligned rows, and var. *patula* Rouy, with leaves which extend out almost at right angles to the stem. The latter variety seems only to occur in moist protected situations and may be nothing more than a habitat modification. Variety *condensata,* however, does not correlate well with the situations of greatest exposure.

Calluna and the gorse, *Ulex europaeus* L. (apparently including some of the subspecies *U. Gallii* Planchon), are both very plastic, old plants in flowering condition sometimes being compactly caespitose and only a few centimeters tall. In fact, on the upper slopes one had carefully to select a place to sit down in the grass for the gorse, although diminutive in stature and obscured in the sward, was just as effective as to needle-sharp branches as were the plants of the lower slopes which were a meter or two tall.

The principal vegetation type of Pic la Rhune may be called Gorse and Bracken Heath because of the prevalence of *Ulex, Pteris,* and the ericaceous plants mentioned above. There is an intermixed grass cover which becomes dominant in the spots of better soil. None of the grass species was found in a condition suitable for identification by one unfamiliar with the Gramineae of the area so, unfortunately, this alternate association must pass without comment. The prevailing heath is undoubtedly what Clements

would term a disclimax, or stable vegetation resulting from abuse. Overgrazing, fire, and deforestation through the centuries have reduced a potential woodland climax to the stable gorse and bracken heath. Sheep, goats, and ponies range the slopes, exerting a considerable pressure on the grass and favoring the dominants because of their general unpalatability. Without considering historical records, there are several reasons for believing that the slopes of Pic la Rhune and similar mountains in the area were once forested. At the picturesque nearby village of Sare there is a three-centuries-old Basque church in which the main supporting timbers, said to be of local origin, are nearly 2 feet square and 30 feet long. No such timber is to be found growing anywhere in the area today. In the second place, it is true in the south of France and in the Mediterranean area generally that overgrazing and repeated fire have caused vegetational deterioration and accompanying acceleration of erosion of the uppersoil horizons. Finally, of course, at this latitude and under the prevailing climate a climatic timber line would lie at an altitude very much greater than any attained in the western Pyrénées around Pic la Rhune.

After we had climbed above the foot of the mountain at Col de St. Ignace and had entered the heath there was only a scattering of a few trees here and there in the bottoms of the valleys. These were *Salix caprea* L., *Castanea vulgaris* Lam., *Sorbus aucuparia* L., *Ilex aquifolium* L., and *Quercus pedunculata* Ehrh., and they were soon left behind. It is rather interesting and unexpected that at one spot about a fourth of the way up the mountain someone had planted two trees each of *Quercus borealis* and *Liriodendron Tulipifera* L., which seemed to be growing well. At the Halfway House we found a fairly extensive windbreak which included *Cedrus Libani* Barrel., *Cupressus fastigiata* DC., *Picea excelsa* Link, *Pinus Strobus* L., *Fagus silvatica* L., *Betula pubescens* Ehrh., *Platanus vulgaris* Spach., *Fraxinus excelsior* L., and *Pinus halepensis* Mill. Above Halfway House only the *Ilex* and *Sorbus* were noted as native and spontaneous trees, and they were rare and depauperate. Although the soil is rather more deep and moist in the vicinity of the windbreak than is general on the mountain slopes, it would seem that reforestation is feasible.

The dominant gorse, *Ulex europaeus* L., displays its papilionaceous flowers with the fragrance of apricot in the late winter and early spring, reflowering in August and September and sometimes even in early winter. The wicked spiny branches are produced in the axils of spiny leaves. In the closely related *U. Gallii* Planchon, which is common on Pic la Rhune and generally in the southwest of France, the branches are often inclined downward and strongly curved, making even more effective weapons than the straight thorns of the principal species. Although *U. Gallii* is confined to the southwest of France, *U. europaeus* is widely spread in western Europe, extending in the north to Denmark and in the south to Northern Italy. In the southwest, west, and center of France the gorse often covers vast areas as the sole dominant among woody plants.

Pteris aquilina L., which is perhaps as nearly cosmopolitan as any species, is abundant at all elevations on Pic la Rhune. It reflects moisture conditions very well, varying from a height of less than one-half meter in the drier more exposed places to as much as two and one-half meters in moist shaded spots in the valleys. No other fern is of widespread occurrence on the Pic although several species are found on moist, protected rocky slopes and cliffs at lower elevations. The following were noted: *Osmunda regalis* L., *Polystichum Filix-mas* Roth, *Athyrium Filix-femina* Roth, *Acrostichum Thelipteris* L., *Polypodium vulgare* L., *Asplenium Trichomanes* L., *Blechnum spicant* Roth, *Ceterarch officinarum* Willd., and *Scolopendrium officinale* DC. Only the *Asplenium* and *Polypodium* were seen among the rocks at the top of the mountain. All of these species are widespread, more or less common in France, nearly all are circumboreal, and most of them occur also in northern Africa. From one to ten or more varieties are known in France for these species, but none was collected on Pic la Rhune.

The students were interested to find the circumboreal sundew, *Drosera rotundifolia* L., in some beds of moist sphagnum and to see for themselves that the glandular hairs on the leaves really were capable of catching

various kinds of small insects. In the same moist place there is a considerable amount of *Juncus conglomeratus* L., a near relative of *J. communis* E. Mey. Near the top of the mountain, incidentally, we found clumps of *J. glaucus* Ehrh., as well as the former species. Scrambling through the heath at all elevations, but never in any quantity, were diminutive, delicate, and very lovely plants of *Whalenbergia hederacea* Rchb. This campanulaceous genus was entirely new to me, and the species seemed most inappropriate among its harsh and more rugged-looking companions. The genus, which is moderately large with its center of development in South Africa, is, I believe, represented in western Europe only by the above species which ranges from the Iberian Peninsula to the British Isles and across France into Denmark and western Germany. Another attractive plant in flower at the time of our excursion was *Gentiana Pneumonanthe* L., which we have found in several other places in southwestern France and which is said to occur eastward to Siberia.

In soil pockets among the rocks and cliffs around the top of the mountain we discovered *Allium fallax* Don in full bloom and the basal leaves of *Oxyria digyna* Hill, which I had last seen in the Snowy Range of the Medicine Bow Mountains of Wyoming at about 3500 meters. This species occurs widely in Arctic and alpine zones around the Northern Hemisphere. At Pic la Rhune it must be at about its lowest altitude for the latitude. In the Alps it reaches nearly to 4000 meters. Among the same rocks, beside the common plants occurring at all elevations on the Pic, we found a little *Vaccinium Myrtillus* L., *Viola canina* L., and *Solidago Virga-aurea* L. The goldenrod seems to differ in no essential respect from that which prevails at the foot of the mountain. On the dunes at Chambre d'Amour and in sandy places elsewhere along the coast we had found, however, quantities of the closely related *Solidago macrorhiza* Lange. It is probably an ectotype of *S. Virga-aurea*. Three species of sedum were collected. We were able to identify only two of them. The most abundant is *Sedum album* L., which occurs widely in Asia, Europe, and Africa, and *S. hirsutum* All. of the Iberian Peninsula, France, and northern Italy.

Some other plants of the upper slopes of the mountain which were in flower and which, consequently, we could identify were *Bellis perennis* L., *Taraxacum Dens-leonis* Desf., *Serratula tinctoria* L. (the juice of which yields a yellow dye), *Anthemis arvensis* L., *Geranium molle* L., *G. Robertianum* L., *V. nummularia* Pourr (of the French and Spanish Pyrénées), *Polygala depressa* Wender, *Scabiosa Columbaria* L., *Brunella vulgaris* L., and *Potentilla procumbens* Sibth.

Our list for the trip was concluded by such common and often weedy plants as *Sherardia arvensis* L., *Peucedanum Oreoselium* Moench., *Digitalis purpurea* L., *Hypericum montanum* L., *H. pulchrum* L., *Polygonum Hydropiper* L., *Trifolium pratense* L., *Verbena officinalis* L., *Ranunculus nemorosus* DC., *Cerastium vulgatum* L., and, of course, *Stellaria media* Cyr.

Although we collected enough to keep us busy for a while and saw many plants that were new for us, the flora of Pic la Rhune is not a very rich one and, generally, presents a monotonous aspect. However, the weather was glorious and the scenery memorably beautiful. Furthermore, we were interested in some demolished German installations. They apparently had operated weather and radar stations on the Pic, and the upper slopes had scattered lookout posts and gun emplacements which were cleverly built so as to blend into the rocks and sod of the terrain at a surprisingly short distance. We even came across the widely scattered remnants of a German fighter plane which had crashed on the upper slopes.

Happy, but tired and thirsty on our return to Col de St. Ignace (one of the soldiers remarked that he certainly would have griped had the Army made him work as hard), we drove directly to the nearby village of Sare and its tavern with tables under pollard sycamores. After a round or two of what seemed like France's best muscatel, we thought even we could understand the strange Basque language, but perhaps it was just friendliness which is understood though the words are strange.

APOLOGIA

By Clarence R. Wylie, Jr.
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There are among us giants whom we know
And knowing, honor; and a few perchance
Cloaked in obscurity to whom our sons
Will look with awe a generation hence,
And marvel that we recognized them not.
But count them all, those who are surely great
And those whom kindly judgment may appraise
As bearing seeds of greatness, and how few,
How pitiably few there are whose work
Time will endorse as more than trivial.

How then can we defend the weary hours,
Tired eyes, and futile thoughts with which we seek
Our small successes? What the urgency
That keeps us diligent beyond our strength?
Cynics have answers that we scorn as lies:
"Rank and its perquisites." "One's name in print."
Damned be the charge! Who claims it past belief
That men should labor at the heart's command,
And count the struggle dearer than the prize
Their uninspired hands may never hold!

It's said the French who fought at Waterloo
Under Napoleon, in their later years
Were strangely silent on the part they played
In that great gamble; and when pressed to speak
Would only say, almost with reverence,
"Oui, j 'étais la," as though the simple fact
That they had served their leader and his cause
To the last hour, was all the fame they asked.

Is it less so with us whose battle flag
Bears the word *Science*, and whose leaders are
Not blustering soldiers but men of the stamp
Of Darwin, Pasteur, Faraday, and Gauss?
This is the urge that keeps us at our work:
An inner oneness with such men as these,
Whose faith is ours and by whose side we stand.

EUGEN STEINACH, 1861-1944: A LIFE OF RESEARCH*

By HARRY BENJAMIN, M.D.

THIS is the story of revolutionary medical research. It is also the story of a scientist who dared to penetrate the primeval forest of human prejudices and sex taboos and who added immeasurably to human happiness. He earned but little gratitude in his lifetime. Through his investigations and discoveries, he laid the cornerstone for a new medical specialty: the endocrine treatment of old age. Now called gerontotherapy, it endeavors to add life to years; maybe, also, years to life. Who knows?

The closing years of the last century had witnessed a profound change in the philosophy of medicine. Medical science had outgrown the preceding period of a purely speculative natural philosophy, and a new era had begun in which life in all its manifestations was interpreted chiefly in mechanistic and chemical terms. Life was regarded as a chemical accident; the human body as an immensely complicated machine subservient to the laws of physics and biochemistry. Such was the new world of scientific thought when Eugen Steinach entered his special field of medical research by publishing the first original investigations that dealt with the physiology of sex. The year was 1894, and Steinach was 33 years old. It was the birth year of a new field of scientific research.

Physiologists already knew Steinach from numerous and varied publications, but it was a turning point in his career when his series of three articles on the "Comparative Physiology of the Male Sex Organs" appeared in *Pflüger's Archiv*. The first of these investigations can be called a "negative" one, and according to Steinach's own statement it was based on an "accident." He had seen an article by Tarchaloff in which the theory was proclaimed that the sexual "embracing reflex" of the frog was due to a nervous mechanism elicited by the tension of completely filled seminal vesicles. Steinach set out to test this theory. He removed the seminal vesicles not only of frogs but also of rats and found that sex instinct and sexual response were in no way altered. Therefore, the seminal vesicles could not possibly have the significance that Tarchaloff had ascribed to them, and so his theory was conclusively disproved.

But if this was not the explanation for the release of the sex instinct, what then could it be that influenced the psyche in this specific manner and led to the characteristic manifestations of sex? This was the question Steinach asked himself; to answer it he set in motion a chain of experiments that determined almost his entire life's work. One novel experiment followed the other, and each was the logical outgrowth of the preceding one. From then on, Eugen Steinach was inseparably identified with the biology of sex. All his previous researches in other fields of physiology receded into the background.

PRE-SEXGLAND PERIOD

Born on January 27, 1861, in Hohenems, a small town in the Austrian Alps, young Eugen grew up in an atmosphere of medicine. His father as well as his grandfather were practicing physicians. The grandfather was also an enthusiastic and successful cattle-breeder, a fact which bore fruit many years later when his grandson did research work on sterility in cows.

After finishing high school Steinach went to Geneva, Switzerland, to study natural sciences and soon afterward to the University of Vienna to devote himself to medicine. In 1886 he graduated and became doctor of medicine. During the following two years he worked as assistant in the Physiological Institute in Innsbruck and published a few minor contributions, such as "Studies on the Blood Circulation of the Kidney"; "Time-Reaction of Temperature-Sensations," and

* The illustrations of experimental animals in this biography are reproduced from Steinach's *Sex and Life* with permission of the Viking Press, Inc., New York. The autographed portrait of Professor Steinach at his desk is taken from Dr. Heinrich Meng's *Psyche und Hormon* with permission of the author and the publisher; i.e., Medizinischer Verlag Hans Huber, Bern, Switzerland.

"Experiments to Measure Temperature and Pressure-Sensation Time."

His deep interest in physiology soon brought the young research scientist under the influence of Ewald Hering, one of the greatest physiologists of his time, and in 1889 we find Steinach as his pupil and first assistant in the Physiological Institute of the German University in Prague. Here began the first period of Steinach's scientific career, a period which can be designated as "presexgland." It extended roughly to 1910, and, up to this time, the only sex-physiological investigation was the one previously mentioned dealing with the male sex organs.

Steinach remained in Prague over 20 years. He became *Privatdozent* two years after his arrival and five years later was named *ausserordentlicher* professor of physiology. In 1907 the University of Prague conferred on him the *ordentlicher* professorship. Steinach, then 46 years old, was able to look back already on a distinguished, though hardly exceptional, career. He had ambition, initiative, and originality, which were particularly demonstrated in 1902 when he created and organized a laboratory for "general and comparative physiology," the first of its kind in German-speaking countries.

It was principally in this laboratory that a great number of various researches were conducted. Among other subjects they dealt with "Visceromotor Functions of the Posterior Roots of the Medulla Oblongata" and "Chromatophore Muscles of the Cephalopods." In "True Contractility in Motor Innervations of Capillaries" (*Pflüger's Archiv,* 1903) Steinach described original observations of electrical stimuli on the circular muscles of capillaries with photographic studies. In 1908 he published a noteworthy contribution on "Summation of Singly Ineffective Stimuli as a General Phenomenon of Life."

More important than these, however, were earlier series of researches on the "Comparative Physiology of the Iris," which appeared in *Pflüger's Archiv* in 1890 and 1892. They reflect clearly the influence, if not the pressure, of Steinach's great teacher Hering, who himself had contributed so much to the physiology of the eye and the problems of vision.

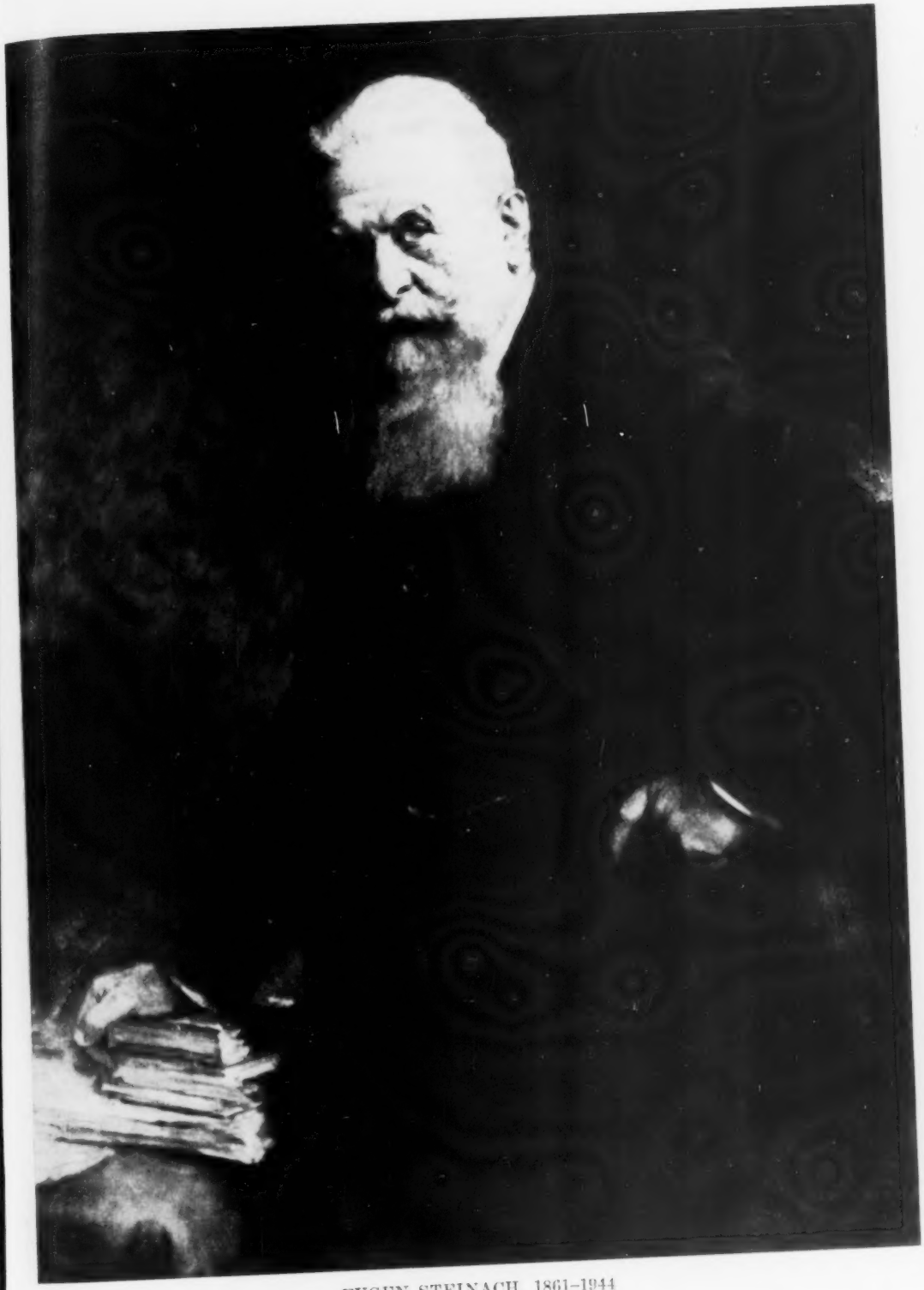
In later years, when Steinach's name had become world famous as a sex biologist, he used to refer to the work in the "pre-sexgland period" with hardly more than a shrug of the shoulders. He considered the experiments "not entirely satisfactory" and once said to his assistant and collaborator, H. V. Klein: "When there is not enough love and devotion to one's work, one can hardly expect much of a success."

Physiology of the Testis

Although this unduly pessimistic attitude toward his own work applied to the first period of Steinach's career, the extreme opposite prevailed in later periods. He was then "heart and soul" in his studies of sex biology to which he devoted more and more time during the later years of his stay in Prague. These studies, which were principally concerned with the physiology of the testis, initiated the second period in Steinach's research life. It was during this time—roughly, from 1910 to 1920—that Steinach's original mind led to his greatest accomplishments and his most brilliant success.

The year 1910 saw the 49-year-old scientist still in Prague teaching physiology and conducting or supervising experiments. Among the latter were already those in sex biology which paved the way for the coming work in Vienna and which laid the foundation of his future fame.

The consequences of castration, well known for a long time, were in these years no longer explained by a severance of nerve fibers connecting brain and testes when the latter were removed. This theory had been exploded. The physiologist von Berthold, by his transplantation experiments on roosters in 1849, had furnished evidence that something in the circulating blood constituted the link between sexgland and brain. Unable, however, to identify the particular agent any closer, Berthold could go no further, and his experiments were soon forgotten. Besides, they had one great drawback that detracted from their scientific reliability: The testes of fowls are most difficult to remove completely, as they are not surrounded by a firm capsule like those of mammals. If, therefore, even the smallest part of testicular substance re-



EUGEN STEINACH, 1861–1944

mained *in situ*, full castration had not taken place and valid conclusions could not be drawn.

Therefore, Steinach used male rats for his transplantation experiments and showed, first of all, more conclusively than ever before, how the gonads influence the secondary sex characters, including the sex instinct. This influence, however, did not mean a complete dependence. The first fundamental publication on the subject appeared in the *Zentralblatt für Physiologie*, 1910, and was called "Sex Instinct and True Secondary Sex Characters as a Result of the Endocrine Function of the Gonads." Although emphasizing the biological pre-existence of male or female sex and their respective secondary sex characters, Steinach showed that growth, maturation, and preservation of the latter were definitely controlled by the internal secretion of the testis. Remove the testes, and the seminal vesicles and prostate shrink; sexual lethargy develops. Implant a testis somewhere else in the body, and the physical as well as the psychic sex characters are restored.

All this sounds trite and commonplace now, but 35 years ago it was an important discovery, or at least a final confirmation of what had so far been mere assumption.

The Puberty Gland. The next logical question that Steinach now asked himself was: In what part of the testis does the internal secretion develop? Is the male hormone a product of the spermatogenetic tissue (that produces the spermatozoa) or is it secreted by the interstitial (Leydig) cells (forming the rest of the testicular tissue)? This question had been asked before, notably by two French scientists, Bouin and Ancel, in 1903. Through vasoligature experiments on rabbits and guinea pigs, they had come to the conclusion that the internal secretion was a function of the interstitial tissue. They called it *Glande interstitielle* and regarded it as the source of the hormone that controlled secondary sex characters. Their theory was accepted in some quarters and rejected in others. Steinach seized the opportunity presented by his own transplantation experiments (1910) to try to settle the question.

He had been the first experimenter who succeeded in transplanting a testis in such a way that it became vascularized and remained functioning in its new site. Infantile animals had been used. When they were castrated, they remained infantile. But they developed normally when the re-embedded gonad remained alive in the body. What kind of a gland was it now, that continued its function somewhere under the skin instead of in the scrotum? Years later, in his book *Sex and Life*, Steinach described the histological structure as follows:

> The spermatogenetic (germinal) tissue proved to be more sensitive and was unable to adapt itself to the altered conditions of blood supply, and therefore perished. The hormonic tissue, on the other hand, proved to be more robust and not only adapted itself readily and completely to the new environment, but also increased. The space made available by the atrophy of the germinal tissue was occupied by proliferating interstitial cells. Thus, as an incidental result of my transplantation experiments in 1910 and succeeding years, I found that I had changed testicular tissue into a gland consisting wholly of interstitial cells.

Since this gland promoted the maturing process, Steinach named it the "puberty gland."

The older, inconclusive experiments of Bouin and Ancel were now evidently confirmed, and the internal secretion of the testis had to be ascribed to the Leydig cells. In taking a decided stand in favor of this theory, Steinach entered the first of several scientific controversies in which he was a leading figure, if not the actual storm center. Objections to the "puberty gland theory" persisted in several scientific circles, where the internal secretion was still ascribed to the spermatogenetic tissue or to the Sertoli cells, which are a part of this tissue.

It would be only of historical interest to go into the details of this controversy, although it played undoubtedly a great role in Steinach's scientific trials and tribulations. It was finally decided in Steinach's favor (and naturally in favor of Bouin, Ancel, and their adherents) when two of his principal opponents withdrew their objections in 1926 and 1933, respectively. Steinach had to wait over twenty years for this recognition, but when it finally came—in a rather dramatic episode—it was one of the highlights in his

career. It was in Berlin in the fall of 1926 when the First International Congress of Sexual Research took place, and Steinach had delivered a lecture on "Sex Hormones, Their Biology, Physiology, Pathology, and Therapy." At that time he was already a world-famous figure in medicine on account of his publication on "rejuvenation" and was the "star speaker" of the Congress. The auditorium was crowded to overflowing. No discussion had been scheduled after Steinach's paper, but Dr. Albert Moll, the president of the Congress, arose and congratulated the speaker on his results, which he compared in fundamental importance to the Mendelian laws of heredity. After him, Dr. Oscar Riddle, distinguished biologist of the Carnegie Institution, who was chairman of this session, spoke briefly to recall the debt which the biology of sex owed to Steinach for his important contributions.

Everybody expected the meeting to close after these remarks, when Dr. Moll rose once more and announced that Professor Benda had asked for the floor to make a brief statement. The famous pathologist of the Berlin University had been known for years as one of the most bitter opponents of Steinach and of his "puberty gland" theory. The large audience expected another scathing attack—this time orally—and the tenseness was distinctly uncomfortable. I happened to be present, sitting next to Dr. Peter Schmidt, Steinach's foremost pupil in Germany, who whispered, "Watch the fireworks."

In this electric atmosphere Benda's words amounted to a near sensation. Instead of dissenting, he paid high tribute to Steinach and stated in so many words that, following new experiments of his own, he was ready to withdraw his objections and concede the correctness of Steinach's claims. When the two scientists then shook hands the applause amounted to a demonstration.

Benda's declaration had shaken the stronghold of Steinach's opposition, but the victory was only complete when the second great opponent, the anatomist B. Romeis, of the University of Munich, likewise revised and altered his standpoint in 1933 (*Klinische Wochenschrift,* No. 42) and in a truly scientific spirit admitted his error.

Let us return to the year 1912 when Steinach's stay in Prague came to an end. His publications had made a sufficiently deep impression in the scientific world to cause his appointment as research director in the Physiological Department of the Biological Institute of the Academy of Sciences in Vienna. At the same time the University of Vienna bestowed on him the title of *ausserordentlicher Professor.*

Steinach was 51 years old when he came to Vienna. With his striking head (on a pyknic body) and his impressive reddish-blond beard, he was soon a familiar figure in Vienna's *Prater,* where the Biological Institute was located. The Viennese called it the "Vivarium." Steinach was still a bachelor. He was a conscientious hard worker, who demanded much from his assistants. In all his scientific work he insisted upon logic in thought, clarity in expression, and uncompromising adherence to the purpose on hand. But away from the laboratory he was a man of the world, full of genuine humor and of a vitality that let him enjoy the good things in life to the fullest. Here, in Vienna, his greatest wish was at last realized: he could concentrate entirely on research and was no longer distracted by obligations to teach students.

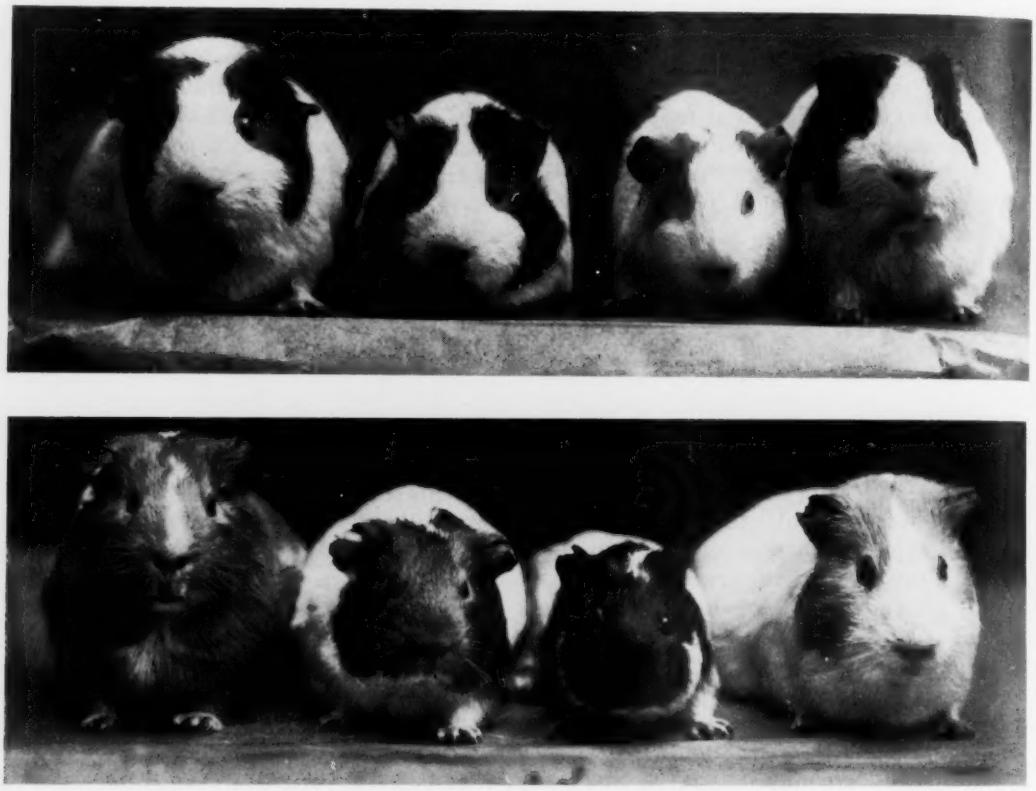
Reversal of Sex Characters. The year of Steinach's removal to Vienna also saw the publication of his classic research work on "Intentional Transformation of Male Mammals into Animals with Pronounced Female Sex Characteristics and Feminine Psyche." These experiments were a natural outgrowth of the former investigations as to the significance of the sex hormones. Were these hormones unspecific as to sex, like the hormones of the thyroid, the adrenals, etc., or were they sex-specific? That is what Steinach wanted to know.

Infantile male guinea pigs were castrated. Into the resulting "neuter" an ovary was implanted. The developing sex characteristics were not those of the congenital masculine *anlage* but definitely feminine. The "foreign" female gland proved stronger than the male *anlage.* Milk-producing nipples appeared; the penis remained rudimentary,

resembling a clitoris. Body growth and configuration was more like those of a control sister than of the normal control brother. The forms of chest and head changed to the feminine type. The rough unkempt hair of the male was replaced by the softer, shorter hair of the female. Even the internal structures of the originally male animal, as revealed by the X-ray, were now like its sister and quite different from its twin brother.

furthermore, were pursued by the normal males that attempted intercourse. They radiated genuine feminine attraction.

The opposite experiment was equally successful and striking. The young castrated female, under the influence of an implanted testis, turned into a "male" and grew a penis; even its voice assumed a characteristic masculine guttural sound instead of the well-known squeak of the female. This



CHANGING THE SEX CHARACTERS OF GUINEA PIGS

Above: MASCULINIZATION. *Left to right:* MASCULINIZED SISTER, SPAYED SISTER, NORMAL SISTER, NORMAL MALE. *Below,* FEMINIZATION. *Left to right:* CASTRATED BROTHER, NORMAL SISTER, FEMINIZED BROTHER, NORMAL MALE.

More astonishing even than the physical transformation was the change in the psyche. Normally, male guinea pigs show no paternal instinct. They repulse the young ones. But the "feminized" male behaves like a mother and actually suckles the young. When Steinach demonstrated these animals and their behavior before a Physiological Congress, they created a sensation. They made medical history. These feminized males, masculinized female, like a normal male, also scents a female in heat, pursues her, and attempts sexual contact. It was this erotization of the brain in the "sexgland-specific direction" that impressed Steinach most and led to many future inquiries into the nature of the sex urge. For the first time the relationship of hormone and psyche was brought into focus. Steinach and Freud (who had met years ago as students) had now found a

contact in their respective researches—the one proceeding from the soma, the other from the psyche. The body-mind problem was moving closer to a solution.

After the appearance of classic "Feminization of Males and Masculinization of Females" (*Zentralblatt für Physiologie*, 1913) Steinach's name as a foremost sexologist was firmly established. The technique of his experiments was impeccable, combining imagination with strict discipline. In many laboratories the experiments were repeated and confirmed. Knud Sand in Copenhagen feminized roosters and masculinized hens. Their pictures can be found together with Steinach's guinea pigs in many scientific textbooks.

Studies of Homosexuality. Numerous investigations and publications followed in consequence and in elaboration of the classic experiments of sex reversal. In collaboration with the renowned Viennese roentgenologist Holzknecht, Steinach tested the effect of X-rays on the ovaries of infantile female guinea pigs (1916) and found that these rays in small ("stimulating") doses caused an acceleration of growth of uterus, mammary glands, and nipples, that is to say, an experimental precocity. In a publication "Puberty Gland and Hermaphroditism," Steinach reported the effect of simultaneous implantation of testicle and ovaries into infantile castrates and described the resulting "experimental hermaphroditism" (*Archiv für Entwicklungsmechanik*, 1916). These investigations were in turn the cause of two further groups of experiments: "Inquiries into the Nature of Homosexuality" and "Proof of the Antagonistic Effect of Male and Female Hormones."

In 1918 (*Münch. Med. Wochenschrift*) Steinach and the Viennese urologist Lichtenstern published a paper with the optimistic title "Conversion of Homosexuality through Exchange of Puberty Glands." This was a practical application of Steinach's researches. Homosexual men were castrated and another man's cryptorchic (undescended) testicle, which consists of predominantly interstitial tissue, was implanted. The homosexual tendencies disappeared and a normal (heterosexual) libido developed. Such, at least, was the conclusion that the authors drew from the report of their patients.

In the light of a modern concept of homosexuality, which sees an endless variety of combinations between homosexual and heterosexual tendencies, and between constitutional and psychological factors, this assumption of an exclusive endocrine etiology of sex inversion and its clinical cure by endocrine therapy must be taken *cum grano salis*. Organic cases of intersexuality may indeed be influenced by glandular methods, but Steinach's and Lichtenstern's generalization is hardly justified.

Another controversy resulted from Steinach's histological examination of testicles of homosexual men, which he described as showing the following abnormal characteristics (*Archiv für Entwicklungsmechanik*, 1920): "Atrophy of spermatic tissue, partial degeneration of the male interstitial cells, presence of large cells, differing widely from the former and approaching in appearance and structure the female cell types." Steinach referred to them as "F cells," but to this day their existence has not been generally recognized.

Antagonism of Sex Hormones. Also in the *Archiv für Entwicklungsmechanik*, 1920, appeared a series of articles dealing with "Artificial and Natural Hermaphrodite Glands and Their Analogous Effects." In the first and most important part, on "Antagonistic and Sex Specific Effects of Sex Hormones," the author comes to these conclusions: Sex hormones are sex-specific, that is, the male hormone develops the *anlagen* of male characteristics, the female hormone doing the same for the female *anlagen*. Hand in hand with this effect on the like sex goes an antagonistic (inhibitory) influence on the characteristics of the opposite sex. Their development is prevented, retarded, or even brought to involution.

Opposition to this theory of the physiological antagonism of sex hormones still exists but remains unconvincing in view of many corroborating experiments, especially those of Alexander Lipschuetz.

The year 1920 also saw the publication

of a rather extensive study "Climate and Puberty" by Steinach in collaboration with the zoologist Paul Kammerer (whose outstanding work in heredity is as well remembered by his colleagues as is his tragic death in 1926). The authors showed that rats kept in higher temperatures developed a proliferation of interstitial cells in their testes and consequently an enlargement of secondary sex characters together with a high degree of sexual excitability. It was not difficult to find the analogy of these "heat-rats" with the proverbial sexual ardor of men and women in the Tropics. Steinach and Freud had met once more.

Rejuvenation. As far as publications are concerned, 1920 was undoubtedly for Steinach not only the most fruitful but also the most fateful year of his research life. In this year appeared his little book *Rejuvenation Through the Experimental Revitalization of the Aging Puberty Gland* (J. Springer, Berlin), and almost overnight Steinach became one of the most talked-of men of science all over the world.

In the medical profession he was soon praised as a genius by some and condemned as a quack by others, while the laity saw in him a modern Ponce de León on the one hand and a violator of sacred laws of nature and morality on the other.

Steinach's thoughts and studies on the problem of old age date back to 1912 when a paper "Investigation of Youth and Age" was submitted to the Vienna Academy of Sciences but remained buried there for nine years. Steinach had observed and, for the sake of priority, described in this early paper the striking resemblance of the effects of castration and those of senility. In both instances the lack of sex hormones produced the similarity of the clinical pictures. The inevitable thought then occurred that both conditions could be remedied by supplying the missing hormones.

The transplanting of a foreign gonad into a castrate had proved successful in curing or ameliorating the characteristic signs of deficiency. Gland transplantation into an aging body was, therefore, the first method to be considered, inasmuch as it had been successful in numerous animal experiments. The injections of hormone solutions in the manner of Brown-Sèquard was another possibility. Steinach did not favor either one. Transplantation would be impractical on a larger scale. Where should all the glands come from? (Voronoff, in Paris, tried to solve this problem by resorting to the use of testicles from anthropoid apes). Hormone solutions were too uncertain in their effects and also difficult to procure in those early days, long before the advent of biologically tested or synthetic androgenic preparations. Steinach wanted a wider human application, not one that could benefit only aging rats and guinea pigs.

Thus arrived the moment when this ingenious scientist had a truly "royal" thought: foreign materials should not be employed to reactivate the senescent system

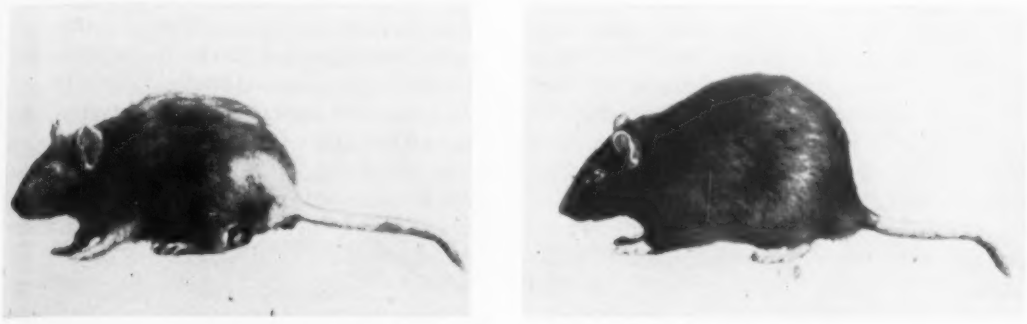
REVIVIFYING EFFECT OF HORMONE TREATMENT ON A RAT

Left: SENILE FEMALE RAT BEFORE ADMINISTRATION OF A FEMALE HORMONE. *Right:* THE SAME AFTER TREATMENT.

but the person's own sex gland should be made to produce the necessary hormone increase. The body's own resources must be mobilized. This is the principle underlying all those therapeutic methods which justly deserve the designation "Steinach therapy."

Vasoligation was the first practical result of this idea, and the small operation soon bore Steinach's name. Bouin and Ancel had touched off nevertheless another scientific controversy, this time especially vehement on account of its practical clinical implications, namely, the claim that vasoligation "rejuvenates" not only animals but men as well, though to a lesser degree.

Steinach had waited for years with this pronouncement, and therefore the storm in the medical world broke only after the afore-



REVIVIFYING EFFECT OF VASOLIGATION ON A RAT
Left: SENILE MALE RAT BEFORE LIGATING ITS VAS DEFERENS. *Right:* THE SAME ANIMAL AFTER THE OPERATION.

used the ligation of the vas deferens previously but for experimental and theoretical purposes only. Now Steinach wanted to apply it in the practice of medicine and to combat old age by taking advantage of its effect on the histological structure of the testis.

The vas deferens is the canal through which the spermatozoa reach the outside (i.e., the urethra). If this canal is sealed through a permanent ligature, the spermatozoa are dammed back into the gland, gradually causing a pressure atrophy of the very delicate germinal epithelium from which they originate. This, at least, was Steinach's reasoning. The place of the disappearing cells is taken by the much hardier interstitial cells which—according to the Steinach school—proliferate and consequently produce more hormone. At the height of the atrophy of one tissue and proliferation of the other the testis was said to resemble a cryptorchic or a transplanted gland.

Although this theory of the histological consequence of vasoligation seemed amply supported by exact laboratory studies, microscope slides, and animal specimens, it mentioned book on rejuvenation appeared in 1920. It dealt at length with all the preliminary animal experimentation but also contained case histories of "rejuvenated" men.

The Theoretical Controversy. The opposition to the "Steinach operation" was twofold. One was directed against the theory of the proliferation[1] of interstitial cells following vasoligation, therefore concerning the laboratory worker. The other was of a clinical nature. The reactivating effect of the operation was denied, for animals as well as men, more often for men only, thereby limiting the debate to the medical profession.

It would be quite impossible to review here the enormous scientific literature that emanated from laboratories and operating rooms all over the world. Among others, Romeis in the *Handbuch der Innern Secretion* (1931) made a complete survey of this literature, and so did Lipschuetz in his book *The Inter-*

[1] According to experiments by Romeis, Schinz and Slotopolsky, Moore, and others, this proliferation is not real but appears to be such only in comparison to the atrophied spermatogenetic tissue. The number of Leydig cells are declared to remain the same although their individual size may increase.

nal Secretions of the Sex Glands and Allen in *Sex and Internal Secretions*. It seems that a majority of the authors generally confirmed Steinach's histological findings, although many found themselves in variance as to explanation and the degree of the changes that occurred in the testicle after vasoligation. In the United States a group of zoologists in Chicago roundly denied that any hypertrophy of interstitial cells took place after the vas deferens was ligated and, therefore, declared not only that Steinach's theory was wrong but also, consequently, that no clinical effects could be expected from the operation. If anyone claimed to have seen such effects, they said, he was just fooling himself and others. This disapproval of Steinach's research claims was the determining factor for a similar disapproval of their clinical application by the American Medical Association.

In justice to Steinach and the many "pro-Steinach" investigators, one should recall an old truth in medicine which can likewise be applied to physiological experiments: Positive findings are of greater significance than negative ones. Positive findings are a reality that call for explanation. Negative findings mean little in comparison. Therefore, it seems logical to ascribe more credence to those authors who saw changes after vasoligation than to those who did not see them.

Several factors may explain the divergence of findings: The technique of the operation, the period of time that elapsed between the ligation and the examination of the testis (under the influence of an increased hormone production from the "puberty gland" a regeneration of atrophied spermatogenetic tissue is said to take place), the type and age of the animal, the method by which the changes were evaluated. In any event, the final word as to the effect of vasoligation on the structure of the testis is still to be spoken.

"Within modest limits, the process of aging can be influenced." Such was Steinach's own summarization of the result of the operation. Two points were emphasized before he recorded the effects: First, the proper technique of the operation that must seal the vas deferens permanently and must preserve blood and nerve supply to the testis. Secondly, the proper selection of the patient. A favorable response would then "revivify" the entire glandular system, and a mental, physical, and, incidentally, sexual invigoration could take place.

Steinach himself never operated, nor did he consider himself a competent clinical investigator. He was the research scientist first and foremost. He recorded merely the objective and subjective changes that clinicians had observed in their patients and depended upon their statistics which gradually began to appear in the international medical literature. The reports were by no means uniform. There were enthusiastic confirmations; there were almost complete denials of any beneficial results; and there were guarded reports with the usual appeals to reserve judgment until more experience had been gained. "Positive" reports were generally based on much larger statistics than "negative" ones, which is understandable from the physician's point of view.

In some reports like those from Russia (Eiber and Uspensky) the benefits on physical strength and endurance of laborers were emphasized, while in the majority of others dealing predominantly with the intellectual classes the restoration or preservation of mental faculties was in the foreground.

A few individual cases received wide publicity, such as that of Professor Adolf Lorenz, the famous Viennese orthopedist, who ascribed his return to active practice at the age of 72, after his retirement, to a successful "Steinach."

Known in Vienna but not published was the fact that during the 1920's well over 100 university professors and teachers underwent vasoligation, among them Sigmund Freud, in order to improve their health and extend "the platform of their efficiency." Freud, as well as others, was emphatic that such had been accomplished, but exact scientific data are missing.

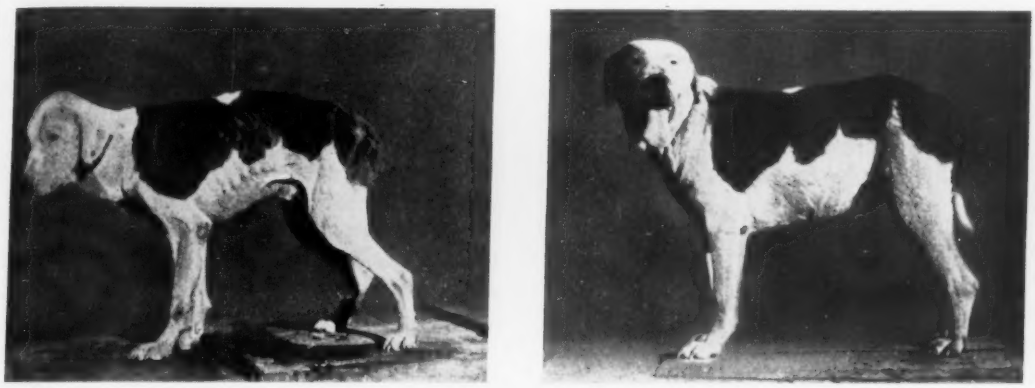
The Clinical Controversy. Exclusive of these honest and independent members of the medical fraternity who reserved judgment as to the value of the operation because they had no personal experience and did not

feel competent to express an opinion, there were roughly three different groups of physicians who took a stand in the controversy: First, there were those who roundly denied any results from the operation, simply because they had not seen any or considered Steinach's theory to be faulty. They pronounced the benefits reported by others as due to "autosuggestion," and the successes with animals were denied or ignored. Secondly, there were those who likewise believed Steinach's theory to be wrong but did not deny clinical benefits, which they ascribed, for instance, to absorption of degenerating spermatogenetic tissue. In accordance with this theory they claimed that the benefit to the patient was only of relatively short duration. Finally, there were those who believed Steinach to be most likely correct in his theory because, with it, they could best explain the invigorating and re-energizing results on body and mind that they had observed in their patients (or some of them in themselves), especially if this result seemed to last for many years.

But the opposition to Steinach was powerful and persistent in its arguments. The term "rejuvenation" easily invited criticism. It was unfortunately chosen, as Steinach found to his regret, for it led to exaggerated expectations of patients and doctors and consequent disappointments, and it produced an unavoidable echo in the lay press and a publicity that wallowed in sexual implications. Later Steinach replaced "rejuvenation" by the more appropriate term "reactivation," which was first proposed by this writer in *American Medicine*, August 1922. The only "sexual" factor in "rejuvenation" was the use of the sex gland. A restoration of sexual potency was incident to the general re-energization of aging men and was only mentioned as such. But this could never satisfy a sensation-hungry press and public or quiet the vaudeville comedians who were untiring in confusing the methods of Steinach with "monkey glands." Consequently more criticism arose, and the serious scientific efforts of Steinach and his co-workers were severely handicapped.

Then came the "Steinach Film" and the wide publicity it created in 1922. A German film concern made a full-length moving picture record of some of Steinach's outstanding experiments. There was an authorized scientific version as well as a popular one. The public loved it, but the medical profession in Germany and Austria frowned. Steinach was raving mad and claimed never to have given consent for the popular form.

Some of Steinach's co-workers indulged in uncritical praise of the effects of vasoligation. Their overenthusiasm and that of laywriters on medical subjects was then rivaled by equally uncritical opposition on the part of some medical editors and authors. Steinach and his collaborators were, for instance, blamed for a publicity and an exaggerated public interest that was clearly due to the nature of their work. Again, in justice to



REVIVIFYING EFFECT OF VASOLIGATION ON A DOG

Left: EIGHTEEN-YEAR-OLD DOG BEFORE VASOLIGATION. *Right:* THE SAME DOG AFTER THE OPERATION (WILHELM).

Steinach, we must remind ourselves that there are always critics in every branch of human endeavor who write their comments not in the light of facts and logic but in the twilight of their individual prejudices, their own likes and dislikes.

Neither Steinach nor some of his pupils can be absolved entirely from responsibility for the fact that the controversy was often conducted in an undignified manner. Occasionally, rejuvenation claims were attributed to Steinach which he had never made (the newspapers had done so), and straw men were set up and knocked down. Unnecessarily sharp rejoinders followed. The acrimony of the debate betrayed its high emotional potential. Steinach, whose vanity was easily hurt, suffered greatly under all these attacks, some of them surely unjust. They left their mark on him psychologically throughout the rest of his life. He became unduly suspicious even toward his friends and easily antagonized people. He was too deeply hurt himself to realize when he hurt others.

Now the controversy over the "Steinach operation" has died down. The development of the synthetic testosterone has reduced interest in the operation. Steinach had the satisfaction of having watched its great popularity and of having heard from many supporters and grateful patients. He had the disappointment of its lack of universal recognition. He himself blamed this failure on prejudices and enmity in the profession, on a frequent faulty technique of the operation, and also on puritanical influences which opposed sex in all its implications, even in utilizing the sex gland for promoting better health in later years. By way of illustration, he liked to show a letter which he had received from a Swedish clergyman who threatened him with Hell and damnation if he continued any longer to interfere with the divine laws of nature.

After the portentous year 1920, Steinach published one more brief paper on the "rejuvenation" operation. He had to take issue with the argument that his rats were suffering from mange but were not old when they were used for rejuvenation experiments; that disease, not senility, accounted for their hair changes. Steinach had to bring proof that these rats were undoubtedly senile, as they had grown old in the laboratory under constant control and that their loss of hair was natural and not due to infection. ("Mange and the Combat of Senility," *Wien. Med. Woch.*, 1921.)

For the following five years no publication came from Steinach's laboratories, although research was never entirely interrupted. Those were the years of monetary inflation and of Austria's greatest economic plight. Government subsidies had practically ceased for all research work, and Steinach had to keep the laboratory with several assistants going the best he could out of his own pocket. Fortunately, patients came to him from all over the world for consultation and operation. Though he did not operate himself (except on animals), he was nevertheless present during all operations, which were performed by such competent surgeons as Robert Lichtenstern and Erwin Horner. In Steinach's waiting room, laboratory, or hospital the rich and the famous from abroad met with the poor and the famous from home. Those were Steinach's busiest years. It was often a problem to do justice to his clinical, research, and literary work and yet allow time for the many visitors, both doctors and laymen.

Steinach was now in his early sixties. A few years earlier he had married the cultured widow of a noted attorney. With her command of English and French she was an invaluable assistant. Who does not remember her as the gentle soft-spoken "secretary" anxious to please everybody, not the least (and not the easiest) the *Herr Professor?* Steinach was sometimes "difficult" indeed. He was a man who made enemies easier than friends. He was touchy, quick-tempered, and—having been "burned" too often—forever suspicious. The *Frau Professor* frequently had a hard time trying to smooth ruffled feelings.

On other occasions Steinach, in his ambivalent nature, was depressed, looked at himself as a martyr, and became resigned to being misunderstood. Fortunately, his moods were of a passing nature, and he was able to forget his irritation and his mistrust, to be again a

sincere friend, a liberal teacher, a sympathetic physician, and a gracious host. A moderate prosperity had come to him, and, being a man who forever said "yes" to life, he enjoyed his hobbies and pleasures fully. He loved horses. For many years he could be seen in the *Prater* on horseback or driving a carriage. He was a handsome man in spite of his short stature. His white hair, his graying beard, and sparkling dark eyes, which could flash so angrily and smile so kindly, were not easily overlooked or forgotten.

BEFORE THE WAR
STEINACH AND HIS FAVORITE HORSE IN VIENNA, 1937.

Biochemistry of the Hormones

The Female Hormone. The third period in Steinach's research career, which roughly extended from 1920 (or 1925) to 1938, began with an interest in the female hormone and in female reactivation.

The use of the X-rays in small doses as worked out with Holzknecht for the maturation of infantile animals had occasionally good results in aging women but was not the final answer to the problem. Nor was it the transplantation of ovaries. "Steinach therapy" (the stimulating "autoplastic" method) had to be replaced or supplemented by a "homoplastic substitution therapy." Moderately effective ovarian hormone solutions had already been made by others, but to Steinach belongs the credit of developing the first standardized preparation of high potency and also of using such female hormone for the purpose of reactivation.

The test for such an ovarian preparation was its ability to "initiate the sexual cycle, to develop sex characters, and to reactivate the senile female organism." This was essentially the title of the publication in *Pflüger's Archiv* in 1925 (Steinach, Henlein, and Wiesner) which described the experiments on female rats, using an extract derived from ovaries and placenta. This preparation could be standardized in units and tested according to the vaginal smear method of American physiologists (Allen and Doisy, Long and Evans, Papanicolaou, *et al.*). It laid the foundation for future work culminating in the production of "Progynon."

After Steinach had succeeded with his preliminary work on the female (now known as "follicular") hormone in attracting the attention of the chemical industry, the work of purification and concentration made rapid progress. It was the Schering-Kahlbaum Corporation in Berlin (Professor Schoeller) that lent help through their own research laboratories, and the result was "Steinach's Progynon," the first purified highly potent and standardized female sex hormone that was put at the disposal of the medical profession. Today's "Progynon," so popular in the United States, is a partly synthetic product. In this respect it differs from the original preparation, but it owes its existence nevertheless to Steinach's original research work, a fact now rarely remembered.

The years 1925 to 1928 were devoted principally to the study and elaboration of the effects of the follicular hormone. If it was injected into infantile males an inhibition of the development of their sex characters took place, while their retrodevelopment was observed in adults. These experiments, published in collaboration with H. Kun, confirmed once more the "Antagonistic Effects of the Gonadal Hormones" (*Biologia Generalis*, 1926).

In 1928 two publications appeared in *Pflüger's Archiv* on the female hormone. The first, written in collaboration with Dohrn, Schoeller, *et al.*, was entitled "Bio-

logical Action of the Female Sex Hormone" and described its influence on the mammary gland (its growth and milk secretion) and on the uterus. The second, with Kun and Hohlweg as coauthors, dealt with the "Reactivation of the Senile Ovary and of the Female Organism through Hormonal Influence." Here the hormone's favorable effect on hair growth and red blood cells was recorded and also the restoration of fertility. As an additional aid for the reactivation of women, Steinach somewhat later advocated the use of diathermy to the ovaries to produce hyperemia. With simultaneous injections of the hormone, substitution and stimulation therapy were combined.

The Pituitary and the Central Nervous System. In the late 1920's the pituitary gland began to assume some of the importance that later researches so fully substantiated. Steinach's interest was readily aroused and led to important studies and conclusions. In 1928 he published a paper (*Mediz. Klinik*) with one of his favorate collaborators, H. Kun, on "The Significance of the Hypophysis as Activator of Gonadal Secretions." With the then crude extracts from the anterior pituitary lobe, the authors induced precocious maturation in infantile male rats. They explained it by the activation of the Leydig cells which were said to increase in numbers. In eunuchoid and senile animals they effected a restoration of atrophic sex characters and concluded that the pituitary was chiefly responsible for the "mechanism of development" and for a "biological law of puberty."

Deviating briefly from glandular studies, Steinach and Kun conducted experiments and published them in 1929 and 1930 (*Mediz. Klinik*) which revealed the presence of a specific *Reizstoff* (excitant) in the central nervous system. This so-called "centronervin" increased the excitability in frogs and the fly-catching ability of tree toads. The authors suggested its possible therapeutic use by employing brain and spinal cord tissues.

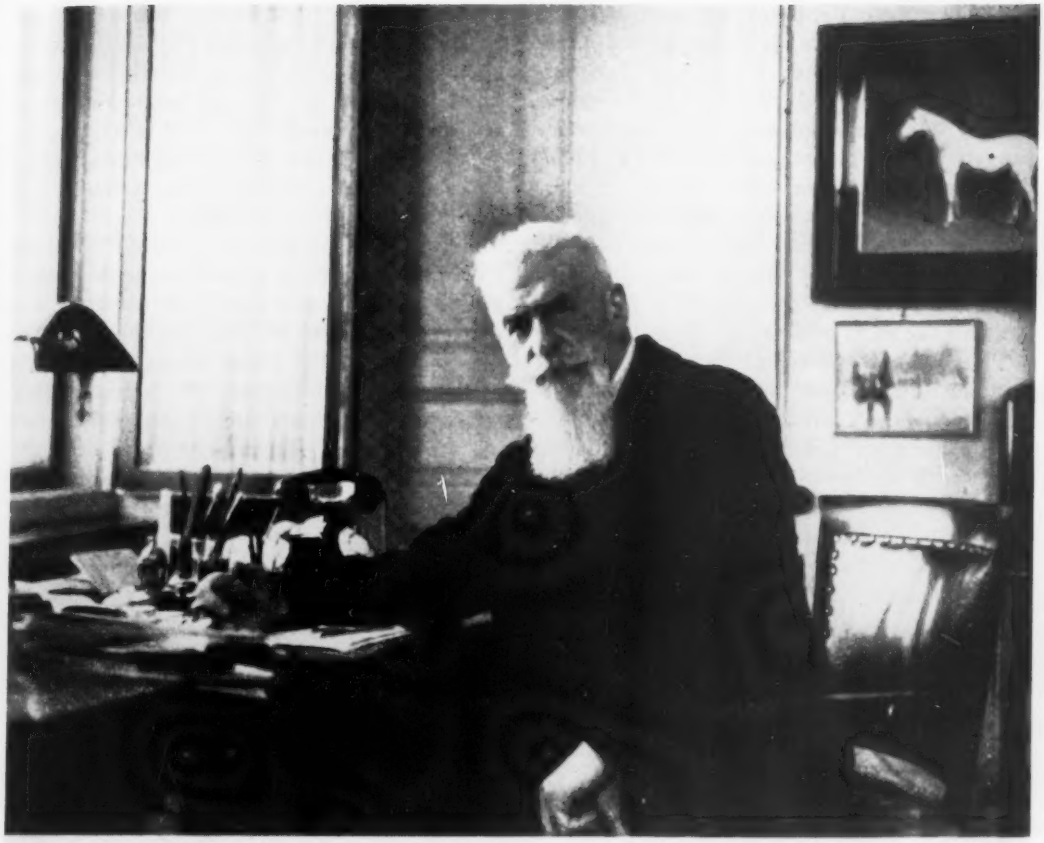
In 1931 Steinach returned to the studies of the ovary, but this time experimenting with the corpus luteum. He found two luteal hormones—one, sex-specific, affecting the uterus; the other of masculine characteristics capable of influencing secondary sex characters just like the male hormone. He spoke, therefore, of the "hormonic bisexuality" of the ovary (*Pflüger's Archiv*).

The Male Hormone. In the meantime male sex hormone preparations had been developed: first, androsterone from urine (Funk and Harrow, Laqueur, etc.) and then testosterone from testicles (Koch, Butenandt, Ruzicka). Steinach tested the latter, stressing the effect on psychic functions (erotization) in addition to the known somatic influences (*Anz. Acad. Wissenschaften,* 1933).

The following year saw Steinach in Switzerland collaborating with the veterinarians Staeheli and Grueter in the "Correction of Sterility in Cattle and Hogs" (*Wien. Klin. Woch.,* 1934). (The grandfather's cattle-breeding had planted the seed for this new interest.) One or two injections of "Provetan" (which was the "Progynon" of veterinary medicine) initiated oestrus in sterile cattle. These original experiments received wide attention, confirmation, and elaboration in veterinary circles.

Several summarizing publications on the "Analysis of Sex Hormone Effects" (Steinach, Kun, and Peczenik, *Wien. Klin. Woch.,* 1936) and on the "History of the Male Hormone" followed. In another series of articles (*Wien. Klin. Woch.*) the hyperemia produced by the sex hormones was especially emphasized. This hyperemia, Steinach thought, was the mechanism through which the results on body and mind must be explained. These studies readily led to the experiments on hypertonia in the same year (*Wien. Klin. Woch.*). A suitable combination of male and female hormone was used for the treatment of hypertension. Elaborate and interesting charts showed the reduction of blood pressure in a number of cases.

Likewise, in 1936, Steinach, Kun, and Peczenik published "Diagnostic Test for Disturbances of Male Sexual Functions Due to Endocrine Causes and Its Clinical Application" (*Wien. Klin. Woch.*). The creatine content of the urine after a test meal was used to differentiate impotence of an endo-



E. Steinach

crine nature from purely psychic impotence. The former would be indicated by an increase of creatine output. The reactivating effects with the synthetic testosterone on men in various age groups were also recorded.

Except for a few minor articles (*Lancet*, 1937 and 1940) dealing with the male sex hormone, only two other publications by Steinach deserve to be recorded, because they appeared exclusively in the United States. One is an article in the *Medical Record* (New York), 1927, "Biological Methods against the Process of Old Age," a summarization of laboratory and clinical data, and the other is his book *Sex and Life (Forty Years of Biological and Medical Experiments)*, which the Viking Press published in 1940. It is a somewhat autobiographical record of his life's work.

Last Years in Exile

Steinach's scientific work ended with the end of an independent Austria. When Hitler marched into Vienna (how easy it is to speak

of it now after VE-Day), Steinach and his wife happened to be in Switzerland. They never returned to their home. At 77, Steinach was an exile, like Freud and so many others. In Zurich, his new home, he bore up bravely under his personal tragedy, which was deepened by his wife's death. Fortunately, he had no economic problems, and he still had his indestructible zest for life, his science, his hobbies. His philosophy of life allowed him to make the least of what he had lost and the most of what he retained.

The Swiss Government had granted him permanent residence, but his enforced idleness was a heavy burden. He suffered under personal and scientific loneliness. His fertile mind with his ever-youthful curiosity was constantly filled with new ideas for experiments, and it was not surprising, therefore, that an old wish to visit America became a new and ardent hope. In America there would be a chance to start research work once more. Many efforts were made through universities, research institutions, and private organizations to have him invited to the United States. Promises were given but not kept. Various excuses were found for not opening the doors to him. Some said he might be too old; others said the physiologist had nothing to offer anymore. "If Steinach would be a biochemist, we would go to any length to bring him here" was one statement. In another instance fear was expressed that too much publicity would result from Steinach's arrival. "You know the American Medical Association. They would not like it," was another statement. And so Steinach never came.

It was his last disappointment. His powerful vitality, which he retained to the last (for he fought senility in himself as he had fought it in others) finally succumbed to stronger forces. A lonely, uprooted, and somewhat embittered man, Steinach died on May 14, 1944, in his eighty-fourth year, during a sojourn in Territet near Montreux. His cremation in Zurich on May 17 was the occasion for impressive services when leading Swiss scientists honored the memory and work of their great colleague.

Like every true pioneer, Steinach had relatively few immediate followers for the simple reason that he was ahead of his time. It matters little that some of his theories have not been verified and some of his experiments were not fully confirmed. Neither were Darwin's, nor Virchow's, nor Freud's. Steinach has nevertheless shown new ways and created new foundations on which subsequent researches were built, although this fact is often forgotten, especially in this country, conveniently by some, carelessly by others.

Professor Heinrich Meng, of the University of Basel, in his recent fascinating book *Psyche und Hormon* emphasizes and conclusively shows the fundamental importance of Steinach's lifework for all sex hormone research and therapy of today.

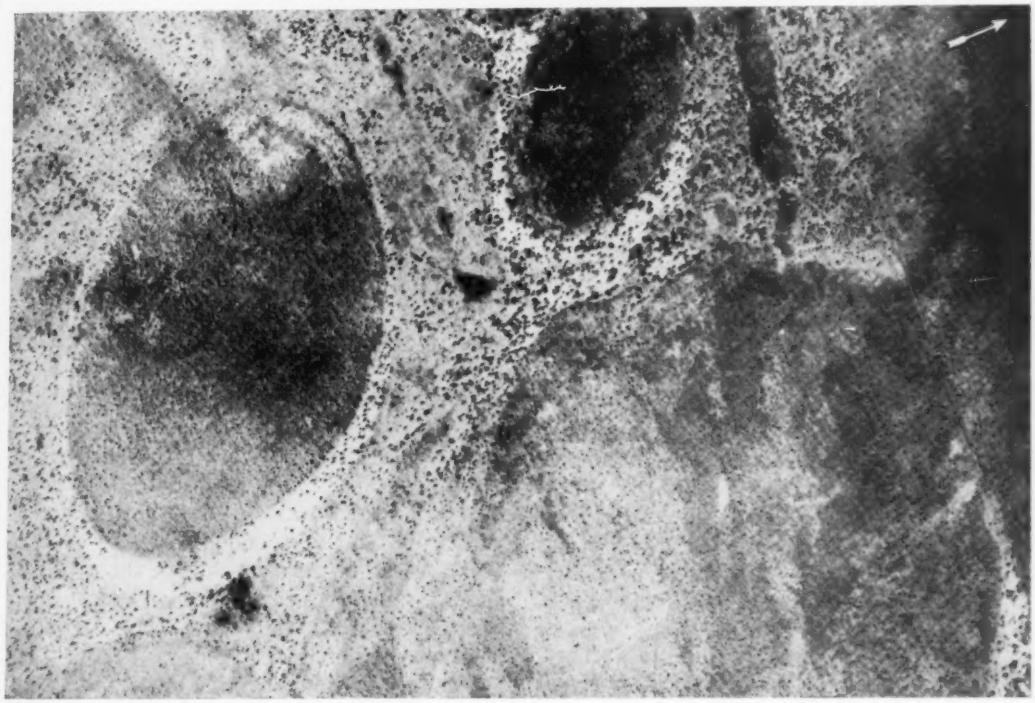
When Steinach approached the "dangerous" problem of sex physiology, all the sex taboos and prejudices of his day were arrayed against him. Many cried "heresy," just as they did in the times of Copernicus and Galileo, of Darwin, Haeckel, and Freud. Man has forever fought against the recognition of his own status in nature, and "original sin" has blighted for centuries the life of the individual. Steinach's work began in the shadow of the Victorian era, but he himself (to speak in psychoanalytic terms) had conquered the castration complex and was able to approach sexological problems with the fortitude and objectivity of a true scientist. "He found a road, because he dared to walk it."

A BIOLOGICAL EXPLANATION OF THE CAROLINA BAYS

By CHAPMAN GRANT

THE coastal plain of the Carolinas is smooth and tilts very slightly southeast toward the sea. It is covered with remarkable scars which are elliptical and parallel. These scars are shallow depressions from a few yards to over a mile in length and can best be visualized by referring to the accompanying illustrations.

Scientists, geologists for the most part, have been endeavoring to explain the origin of these scars which we will call by their vernacular name "bays." Two types of explanations have been used: the terrestrial and the extraterrestrial. The former variously utilize volcanism, wind, ocean currents, wind-caused currents, lakes, sink holes, drowned bays, raised pitted sea bottom, segmented lagoons, gyroscopic effect of rotating water or a combination of several of these for their explanation. The latter group base their explanation on bombardment by meteorites or the head of a comet. Not one or any combination of these theories satisfactorily explains the shape or the parallelism of the bays. Of them all, the most easily disproved are those attributing the origin of the bays to volcanism or meteorites.



BAYS NEAR MYRTLE BEACH, S. C.[1]

ARROW POINTS NORTH. *Left:* TYPICAL PERFECT BED ½ MILE LONG. NOTE GREATER SAND RIDGE ALONG EASTERN SIDE AND SHARPER CURVE OF NORTHEASTERN QUADRANT. *Below, right:* TYPICAL LARGE INDEFINITELY SHAPED BED.

In the following pages we disclose a third

[1] All illustrations of this article were reproduced from Johnson's *The Origin of the Carolina Bays*, by permission of Columbia University Press; i.e., his figures 10, 27, 29, and 34, respectively.

type of theory, which has never been advanced and which seems to explain all the peculiarities existent in the bays.

Before launching into our hypothesis it is necessary to take the reader back to the not very distant past, geologically speaking, and visualize the kind of territory with which we are dealing.

The Coastal Plain was barely submerged, so that at high tide there was very shallow water over it. We will henceforth refer to the submerged plain as the "shallows." The tides rose and fell in a northwest and southeast direction. There was a dominant wind which blew from the northwest over the northern half of the area and a wind from a slightly more northerly direction blew over the southern half. The divergence of directions of the wind was due to the configuration of the Appalachians. The surface soil was as it is now found, but what is not seen today, or then either, is the subterranean layer of impervious material under which is an open or sandy water-bearing stratum originating at a higher level and possessing considerable hydrostatic pressure at sea level. The shallows received some surface runoff and in addition it had numerous artesian springs welling up through holes in the impervious layer. These springs were of various sizes, but not necessarily large.

To summarize: we had a fairly smooth, very gently sloping shallows covered by a very thin sheet of sea water gently flowing and ebbing in a northwest–southeast direction. There were many artesian springs of fresh water welling up into this sheet of sea water. There was a dominant northwest wind. Nothing to sorely tax the imagination. The waters adjacent to Yucatan present similar characteristics today.

Let us now look offshore at the fish that were there. There were several species and subspecies of vigorous fish which reproduced similarly to the present-day salmon. There were great shoals of these fish, but we do not have to visualize greater numbers than the present-day menhaden or salmon. In lieu of ascending rivers, these fish sought brackish water on the shallows in which to spawn. Some needed fresher water and were tied to the location of springs; others seem to have been satisfied with the water at any part of the shallows. These latter might have been to the former as the present-day trout is to the salmon. Some needed a greater depth of water for spawning beds than was to be found over the shallows in the vicinity of the springs. The fish that congregated over the northern half spawned in elliptical shoals whereas their southern congeners favored oval-shaped schools. The species which were more tolerant to salt water have left no clue as to what regular shape, if any, their spawning schools assumed. It is possible that some species were dependent on a certain amount of sand for spawning. This sand was for the most part mixed with the surface soil, having been brought up by the artesian springs.

The stage is now set for our biological spawning-bed hypothesis.

HEREAFTER we will call the bays "beds," but when quoting Douglas Johnson[2] we will of course use his term, "crater."

Let us inspect the preparation of a spawning bed by a large shoal of fish that needed a constant supply of spring water.

At the proper season and with the proper tide, the fish made their way through the shallow water until they found upwelling fresh water. Here the shoal halted, and if their agglomeration had been broken in their travel, they reformed into a compact elliptical mass with its head near the spring and its longitudinal axis in the direction of the flow of brackish water. Now they started to excavate a bed deep enough for the successful hatching of the eggs and for the propagation of the fry. Possibly they had to have sand to prevent the eggs from becoming smothered in mud or silt.

The excavating was done either by mouth or fins or both, as is done by various species of fish today. The fish worked close together so that the agitation of their vigorous tails would join to produce a current sufficiently strong to carry off the silt and mud. This work was aided by the flow of the tides. The fine material was carried away, but some of the sand settled at the sides and rear of the

[2] JOHNSON, DOUGLAS. *The Origin of the Carolina Bays.* Columbia University Press, 1942.

excavation where the current made by the fish slacked off. Much of the sand remained at the bottom of the bed to receive the eggs. Thus tons of silt could be moved as by a conveyor belt, or the work might better be compared to a bucket brigade.

The fish would have exactly faced the current of the tides but for one reason: the water was so shallow and the prevailing wind so strong that the resulting waves forced the fish to face into the wind. The ebb of the tide was slightly east of the direction of the wind, and hence we find more sand piled at the southeast quadrant than at the rear of the ellipses. When the excavation had reached the proper depth and there was a suitable layer of sand, if needed, spawning commenced. Parenthetically the amount of energy dissipated by the present-day salmon in ascending a stream would excavate as much or more than we have to account for in the beds. Now whether the adults remained over the nest to fan their eggs or whether they returned to sea or whether they died at or near their beds is indiscernible and immaterial. Whether the bed was used on the succeeding year by the same adults or later on by the young that had been hatched there is also immaterial. Suffice it to say that whatever school used the same bed the next time, they occasionally encountered a wind from a slightly different direction. The head of the school had to take up the identical position that was occupied before because of the position of the spring. The prolongation of the school had to be parallel to the direction of the wind, like a blimp at a mooring mast. For this reason we find superimposed beds with axes showing some deviation from one another. When this change of direction did not occur and the axes of successive occupations were parallel, we frequently find concentric rings at the offshore end of the beds. The rings are explained if the successive schools varied in size from year to year.

It appears that there was another species of fish that formed schools of a smaller size and needed less fresh water or possibly they were content with the water as found over the shallows. These schools sometimes started excavating near an occupied bed and even cut into them. Since all cutting was forward, or northwest, it appeared that the trespassing school would cut into the rear quadrant of the school to its front. When the original school was thus invaded, its individuals, seeing fish of another species or at least of a different school approaching from their rear, gave way. The infringing school retained its normal shape, but the rear of the forward school was dented, so to speak. In the few cases where the reverse is the case the explanation is that the bed trespassed upon was not in use at the time.

Beds were abandoned for various reasons such as failure or insufficiency of a spring, breaking up of a school at sea, destruction of spawn by invasion of mud from an adjoining bed, insufficiency of sand or change in tidal currents due to the damming effects of other beds or changes in climatic conditions.

Study of the aerial photographs shows that only a few of the enormous number of beds were used in one season as attested by their varying degree of obsolescence. The dim, imperfectly shaped depressions were beds of a different and probably smaller species that did not need the depth or the elaborate preparation required by the species of which we have been writing. Another possibility is that these dim beds were feeding grounds where possibly a different class of fish dug for its food. Also many of them may be the remnants of ancient beds disfigured by adjacent excavation or partially obliterated by erosion.

JOHNSON has made a more thorough study of the bays than anyone else. He has made a list of 29 features of the "craters," as he terms them, which must be explained to adequately account for their origin. At the risk of repetition we will take up his list in the order in which he has made it and answer it item by item.

1. Many of the craters are of remarkably perfect oval outline.

This is one of the points that made me believe that the origin of the beds was biological instead of geological. Flocks of many species of birds and schools of fish assume formations peculiar to the species. Here we have a modified case wherein the school

crowds together for the greater force given to the current. The elliptical form is for the purpose of continuing the current and of taking advantage of the current already put underway by the forward elements. The width of the ellipse is limited by the ability of the fish to tolerate salt water, the salinity increasing outward from the spring. The fish packed close together for mechanical reasons, for reproduction, for safety, and because of herd instinct. Any aggregation of animals moved by a common purpose and possessing a herd instinct would theoretically form a circle, all headed in the same direction, of course, but actually slightly longer than wide due to the shape of the individuals forming the mass. In the present case we have the individuals facing the same way because of current and wave, limited in width by the diffusion of fresh water, and packing close together for better efficiency in excavating and because of herd instinct. These components result in a nearly perfect ellipse.

2. In some localities the ovals are prevailingly elliptical, in others distinctly ovoid or egg-shaped.

Again a feature that cannot be explained by geological origin. The fact that the southern area has the ovoid-shaped beds and the northern ones the elliptical, with a few scattered ones intergrading and trespassing somewhat within the other area, is very easy to explain by postulating two subspecies that form schools of different shapes, each occupying its own area with some intergradation as is always found in subspecies. The southern species may have found greater efficiency in funneling or narrowing the excavation toward the rear, but otherwise reacting to the same stimuli as the northern schools.

3. Many craters are highly irregular in outline.

This is easy to explain by a biological origin. There are several types of irregular beds. Some are obviously very ancient beds that have lost their perfect form to a great extent by erosion and defacement by adjacent beds. Another type was obviously formed originally with an irregular outline. I attribute these to the same origin, namely spawning grounds, but made by a different species of fish which apparently did not need a spring of fresh water, but were satisfied with the brackishness of the entire area. They could therefore select any place without reference to sand, springs, or any feature except possibly depth of water. They apparently excavated to some extent and the following school selected the same general locality and did a little more haphazard excavating.

4. All gradations in form exist, from craters of the most regular outline to those highly irregular.

True in a sense only. They can all be classified in two categories; those that were originally regular and those that were originally irregular. The intergrades are always due to infringement, age, or other physical reason.

5. Oval craters have axial trends almost always directed between south and east, most of them ranging between S. 10° E. and S. 55° E.

In this statement Johnson grouped all the beds of the north and south areas together. The figure on page 447 shows two distinct groups of directions for the north and south groups respectively. See next answer.

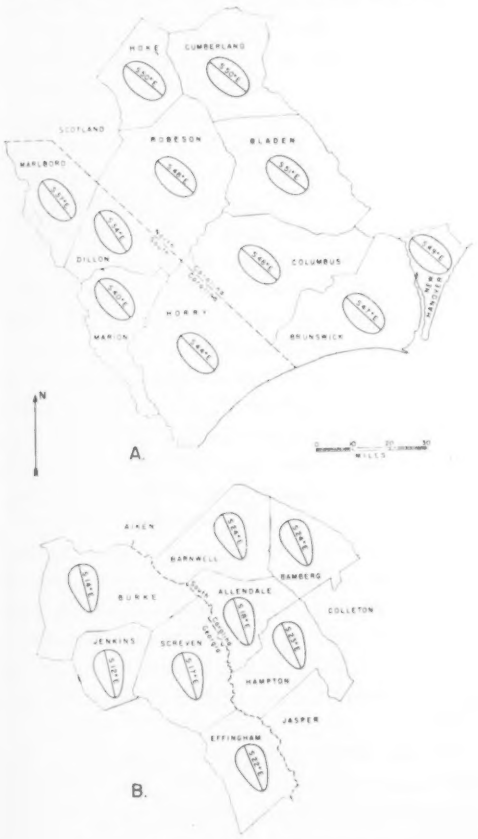
6. There are wide departures from the prevailing direction; elliptical craters are the most consistent in trend, with axes directed more or less nearly southeast; ovoid craters are most variable in trend, but as a rule have axial directions more nearly southward, their narrow ends pointing in this direction.

Again I refer the reader to the above-mentioned figure. Since my hypothesis is based principally on wind or tide direction and the two groups of directions are geographical, I believe that the explanation is that the dominant wind was from a slightly different direction in the two areas due to the configuration of the Appalachians. Also a possible different direction of flow of the tides might add an element to the result. The present general slope of the entire region may be as it was then or it may have warped. Thus the present slope and the present wind may differ from what it was.

7. Craters vary in size: some are a few hundred feet, many one or two miles, and some three or four miles in longest diameter.

Schools of fish vary in size. No stretch of the imagination is necessary to visualize schools larger than those we have today. However, I believe that several species of fish are represented by the various types of beds.

Johnson may not have thought the following problem of import, but I add it here: Very large beds are not as wide proportionately as the smaller ones. The reason is that the volume of fresh water was insufficient to spread over a wide area. Also very large schools of fish tend to become proportionately longer than smaller schools.



BAYS, NORTH AND SOUTH

SHOWING DIFFERENT AXIAL TRENDS AND DIFFERENT SHAPES OF BEDS IN THE NORTHERN (A) AND THE SOUTHERN (B) PARTS OF THE "BAY" AREA.

8. The craters are remarkably shallow in comparison with their great areal extent.

If the beds were deep, my hypothesis would lose much strength as the parallelism of the beds would depend solely upon the direction of tide flow since disturbing wave action would not be felt at any considerable depth. As it is, it is accounted for by the fact that the fish did need greater depth than existed over the shallows, but they did not excavate more than necessary. When the optimum depth was reached, provided a sufficient proportion of sand had beeen exposed, the excavation stopped and spawning commenced.

9. The craters descend below the level of the surrounding plain and below the bases of their bordering rims, as if they were depressions caused by removal of the Coastal Plain material.

Certainly. The excavation was done by the concerted efforts of the school aided somewhat by the flow of the tide. The tide flowed over the top of the rim.

10. Both large and small oval craters show considerable variation in degree of ellipticity.

True, but not more than would be expected in a fluid school that would be influenced by the vagaries of tide, wind, encroachment of other schools, texture of soil or any combination of these causes. Note that the smaller the bed the more nearly perfect it is.

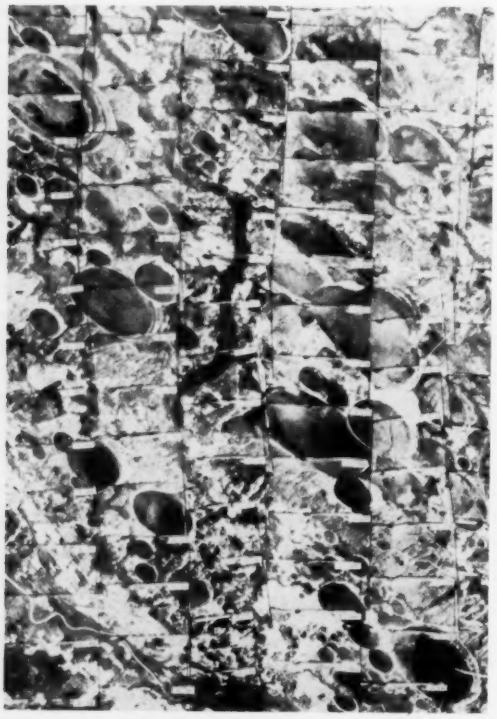
11. Oval craters normally are not true ellipses or true ovoid forms but show systematic bilateral asymmetry, the northeastern sides usually being more sharply curved than the southwestern.

The rising tide from the east was hampered by a quartering head wind from the northwest; the tide ebbing toward the east was accelerated by a following quartering wind. The net result was to carry more of the fresh spring water, that had a tendency to rise, to the northeast side of the long axis of the ellipse than in the opposite direction. Wherever there was water of proper density the fish would excavate provided the eccentricity did not disrupt their close-packed formation.

12. Craters sometimes occur in systematic groups, with a distinct group pattern apparently determined by some pre-existing topography of structure.

The schools selected sites where the conditions were most favorable. Water depth, springs, and sand were the most important

factors, and all three were very likely to occur in close association with topographical or structural factors. This question was included to confound those advocating a meteoritic hypothesis, because meteorites cannot select their striking ground.



PART OF BLADEN CO., N. C.

NORTH AT TOP. MOSAIC AERIAL PHOTOGRAPHS. NOTE PARALLELISM AND APPROXIMATE UNIFORMITY OF SHAPE OF BEDS. NOTE THAT BEDS ENCROACHING FROM THE REAR RETAIN THEIR PERFECT OUTLINE AT THE EXPENSE OF THE BED TRESPASSED UPON. NOTE MULTIPLE RIMS AROUND SOUTHEAST ENDS OF BEDS. NOTE MANY DIM, LONG-UNUSED BEDS WHICH HAD BEEN LOSING THEIR OUTLINES AT THE TIME OF EMERGENCE. BED IN SOUTHEAST CORNER 2¼ MILES LONG.

13. Oval craters, while abundant on parts of the Coastal Plain, are apparently absent from the Piedmont and other Appalachian provinces.

Another poser for the meteorite hypothesis, but hardly calling for a biologist's reply.

14. Many craters are bordered by rims of sand; but . . .

Large rims of sand are doubtless due to the fact that the area excavated contained a large proportion of sand and in order to attain sufficient depth, some sand had to be expelled from the bed.

15. Many craters similar in all other respects have no rims associated with them.

Where no sand rim occurs, it is evident that the low sand content at the site of excavation was only sufficient for nesting purposes. (Note: The irregular craters never have sand rims. I believe that the species excavating them was more tolerant to mud and did not need to be near a spring and could therefore select a location where the water was deeper and needed less excavation and hence no excess sand was exposed.)

16. The rims, when present, rarely completely surround a crater; incomplete rims are sometimes erratically distributed but normally are highest and broadest about the southeastern quadrants of the depression.

The rising tide flowing west was impeded by a quartering head wind from the northwest; the falling tide flowing east was accelerated by a following quartering wind. Therefore the eastward falling tide had more eroding effect than the rising tide. Thus sand brought into the bed from the southwestern rim would have a tendency to be picked up by the fish and passed back or laterally. Sand carried over the northeastern rim was there to stay. The weak rising tide carried less sand over the southwest rim. Erratic distribution of the rims was due to different proportions of sand at different spots of the surface which was being excavated.

17. Multiple rims, nearly but not quite concentric, and from two or three up to six or eight and possibly more in number, occur about some craters.

This shows use of the same bed at successive times, probably by the fish that hatched there, after an interval of several years.

18. Such multiple rims tend to be best developed and farthest apart toward the southeastern ends of oval or ovoid craters, converging or merging or disappearing toward the northwestern ends. In some cases the multiple rims are chiefly confined to the eastern sides rather than the southeastern ends of craters.

The upwelling of artesian water fixed the anterior focus of the bed. A slight deviation

of wind direction or current and especially a difference in size of the school accounts for the multiple rims and their divergence. Probably concentric rings were not built at a single occupation of a bed. A current with an easterly set must account for the occasional occurrence of sand predominating on the eastern rim.

19. A wide space sometimes intervenes between an outer rim or series of rims and an inner rim or series within the same crater; this distribution pattern may be repeated in adjacent craters.

Showing a decrease in the size of the successive schools, owing to lack of food or other adverse conditions. The fact that adjacent beds had similar modifications shows that they were occupied at the same period and that the causes were widespread.

20. The rims are of relatively insignificant size, the volume of material contained in them being but a small fraction of the material removed from the craters.

Showing that most of the spoil was fine material which was carried away by the tide and the current caused by the fish. It also shows that there was not a large proportion of sand in the excavation.

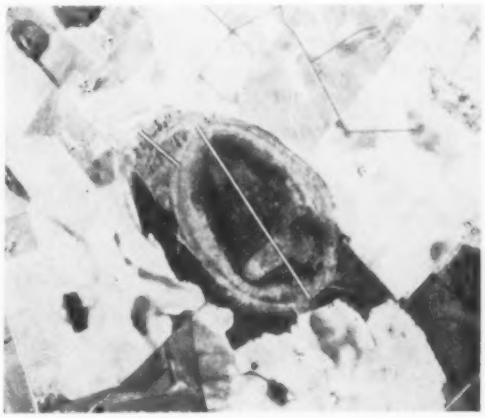
21. The convergence of multiple rims of the same crater and the junction of contiguous rims of adjacent craters do not give rise to combined rims of unusually large size. On the contrary, such combined rims may be unusually small and may locally disappear.

This is a double problem. First, convergence of multiple rims of one bed not showing added bulk. The convergence is always at the forward end where less total excavation is done and hence less excess sand is found. Sand was passed along with mud to some extent from fish to fish, and the excess tended to move rearward more than laterally. Second, junction of contiguous rims not adding bulk, but occasionally being of less bulk or disappearing. Johnson did not record the fact that where one bed irrupts into another it is almost always the rear bed which retains its perfect form and the forward bed which is deformed. This is accounted for by the fact that a school opening up its bed worked forward to some extent and probably also backwards. Thus beds infringed at times. When two schools found themselves together, the rear fish of the front school, seeing strange fish approaching from their rear, gave way. The trespassing school retained its normal form, but the advanced school shrank away sufficiently to leave a boundary of comparatively still water where the sand that was passed back settled. This outlined their warped bed and left a perfect outline around the rear bed. The sand that had been accumulated at that sector by the advanced school was appropriated by the trespassing school and passed back along with sand excavated in their own bed.

22. There is no systematic relation between the size of the bays and the size of their bordering rims. Large bays may have small rims or none, and small bays may have large rims.

Again a result of differential sand contents in various localities. There is also a possibility that different species might have achieved different results due to differing necessities.

23. Many rims are relatively flat-topped or broadly rounded in cross section, frequently with steeper slopes inward toward the crater and outward toward the adjacent plain.



COMPLEX BAY, LEE CO., S. C.
WHITE LINES INDICATE DIVERGENT AXES OF OLDER AND NEWER BEDS. ASCRIBED TO CHANGE IN DOMINANT WIND DIRECTION BETWEEN SEASONS OF OCCUPANCY.

The flat top doubtless represents the water surface at low tide. In cases where the wind built the sand ridges higher than the tide

it is possible that a bed would have to be abandoned because of lack of circulation.

24. The composition of the rims is remarkably uniform, the material consisting for the most part of clean, fairly coarse white or buff quartz sand.

Apparently the sand came up from a lower stratum by action of the artesian springs as Johnson has pointed out.

25. Material composing the Coastal Plain in which the craters are excavated is often strikingly dissimilar to the material composing the rims.

The converse of the last question.

26. Bedrock fragments of Coastal Plain sediments are not found in the rims.

Another question to refute the theory of meteoritic origin. Obviously fish could not bring up fragments of bedrock.

27. Coastal Plain sediments normally remain undisturbed in a horizontal position below the sandy rims and in the crater walls. Occasionally there is slight indication of slumping inward toward the craters but no uplifting of beds causing them to dip outward away from the craters.

Another question to refute the meteoricists. Their hypothesis demands an upwarping. The slumping inwards shows to what extent the artesian springs excavated material from lower levels.

28. Many craters have channels draining into or out of them.

A study of Johnson's photographs makes it appear that these channels developed after the shallows had become the Coastal Plain. Hence they do not enter into our problem.

29. These channels frequently cut across rims bordering the craters.

As the shallows became exposed the surface water found natural channels. Where this flow was obstructed by a sand ridge the water seeped through and excavated from the down stream side, finally gnawing its way back through the rim. I have seen this occur where an ebbing tide cuts through a sand spit which long shore currents had built on the flowing tide. There is no evidence that the rims were cut while in use.

JOHNSON concluded his list by saying: "Such is the array of facts which must receive reasonable explanation before any hypothesis of crater origin can receive general acceptance."

I hope that I have given a reasonable explanation of his 29 facts. I have added two more facts with their explanations under Nos. 7 and 21.

My hypothesis rests upon the vulnerable assumption that all the beds of typical shape used for spawning were relatively shallow and that any deep ones are due to subsequent cave-ins or sinks due to solution or subterranean erosion. Any deep spots in a bed must be accounted for in the same way. The existence of any considerable depth of water that would remove the effect of wave action, which kept the fish facing the wind, would seriously impair my hypothesis.

This hypothesis is so divergent from any so far advanced that geologists at least will hear it with skepticism. Ichthyologists may be more lenient.

IS THE GIANT CATFISH, *SILURUS GLANIS*, A PREDATOR ON MAN?

By E. W. GUDGER
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SHARKS are flesh-feeders and have large mouths and gullets. Indeed, in most sharks mouth, gullet, and stomach form a continuous tube of approximately the same caliber to make easy the ingestion of large masses of food. If the prey is of moderate size in proportion to the size of this food tube, it is ingested whole. If it is too large for this, then it is cut into suitably sized fragments by the large, triangular, saw-edged teeth, or, in sharks having long, pointed, conical teeth, the prey is torn into fragments of a size available for ingestion.

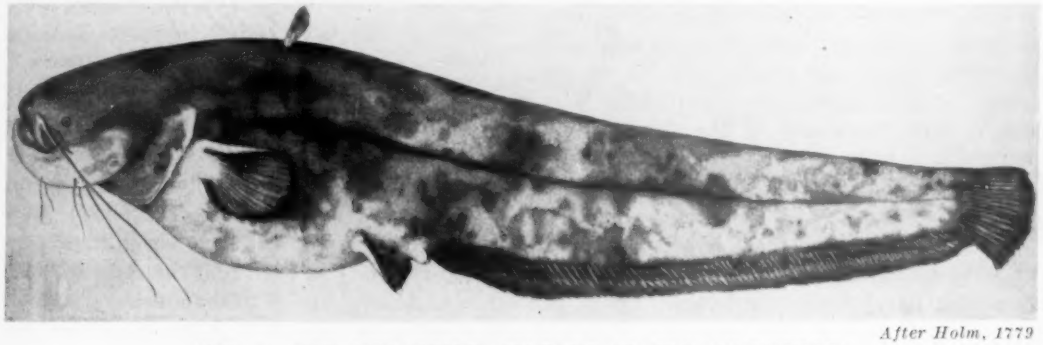
Teleostean (bony) fishes do not ordinarily chop or tear up their prey for ingestion, but generally they swallow smaller fishes whole. Occasionally, however, a bass, pike, trout, or some other of the fiercer forms will try to swallow a fellow fish too large for him and will be choked to death by his prey.

Catfishes, which grow to large size, have huge heads and correspondingly large mouths, gullets, and stomachs. These fishes are voracious and indiscriminate feeders and can swallow large prey.

I have long known that *Silurus glanis*, the catfish of the Danube and other large rivers of eastern and northern Europe, grows to a large size. And I have read that it is very voracious and indiscriminate in its feeding, even ingesting human remains, particularly of small children. Thus D. S. Jordan (1905) says: "It [*Silurus glanis*] is very voracious, and many stories are told of the contents of its stomach. A small child swallowed whole is recorded from Thorn, and there are still more remarkable stories, but not properly vouched for."

Recently I have gone extensively into the natural history of this great catfish and have gathered data not only on its size but also on its feeding habits and particularly on its alleged predilection for human flesh. Whether these data are reliable can only be discussed when they have been set out and studied, but certain it is that they arouse interest in the behavior of this predacious fish.

Silurus glanis has a number of colloquial names, which will be used herein. It is called Wels and Schaid by the Germans and German-Swiss, Sheatfish by the English, and Glanis by others.



After Holm, 1779

FIG. 1. THE SHEATFISH, *SILURUS GLANIS*, OF EUROPE

Most large fishes have large mouths, and such fishes of necessity are apt to be both voracious and omnivorous and to feed on sizable prey. Few teleostean fishes have heads as large and mouths as wide in proportion to the size of their bodies as the siluroids. And so, in keeping with its great

bulk and wide mouth, the Glanis is extremely voracious and equally omnivorous.

Being a big fish, it normally feeds on other fishes. In the stomach of a relatively small Wels, Holm (1779) found 19 fishes—the largest a foot long. The Glanis feeds also on frogs, crustaceans, insects, and worms. In fact, almost anything is grist that comes to its mill. The omnivorous voracity of this fish is embodied in an old Bohemian proverb —"One fish is another's prey, but the Sheatfish eats them all."

Figure 1 is a copy of the oldest and best figure of the Glanis that has been found in this study. It is from an article on *Silurus glanis* by Theodore Holm (1779). Attention is called to the heavy, logy body and particularly to the wide mouth and large abdomen—structures that make possible the heavy feeding to be described later. The size of these parts is emphasized by F. A. Smitt (1895) when he states that the breadth of the head across the opercula is the greatest width of the body and that in old specimens it is about 14 percent (one-seventh) of the length of the body. Then of the mouth he says, "The great breadth of the gape, the width of which measured straight across the corners of the mouth when closed, is about four-fifths of the greatest breadth of the head, and occupies almost the entire breadth of the snout." These statements are all very helpful in getting an idea of the size of these organs, but more to our purpose, especially in view of what is to come, would be actual measurements of a large Glanis.

The indiscriminate feeding of the Wels is attested by various authors. Their accounts have (in part) been summarized by F. A. Smitt (1895, p. 701), who begins as follows: "From older writers we have accounts of assaults made by the Sheatfish on higher animals. Gesner tells us . . . that geese, ducks [evidently caught while swimming] and animals that were being watered [at a river or lake] have been seized." Smitt states also that "long ago" a man standing on the shore of a lake in Sweden saw a Wels seize a lamb which was drinking at the water's edge. Finally, there is the statement by Heckel and Kner (1858) that in the stomach of a large Wels, taken at Vienna, was a poodle dog.

But, according to the old natural historians, the Sheatfish did not content themselves with ducks and dogs. These men go further and allege that man and his offspring are also sometimes devoured by the Glanis. Bloch (1785), who cites his authorities by book and page, quotes Aldrovandi (1613) that near Pressburg in Hungary one of these large fish had swallowed a child, which was bathing in the river, and that, shortly afterward, this or another Sheatfish was caught and in its stomach were found some fragments of a child's body. Bloch states also that J. G. Richter (1754) alleges that near Limritz, in Pomerania, there was taken a great Schaid which had a mouth and gullet so large that it had swallowed a child of seven years. Cuvier and Valenciennes (1839), who likewise document their citations by author, book, and page, state that in 1700 a Wels was taken at Thorn on the Vistula River and that when opened an entire infant was found in its stomach.

Furthermore, according to some of these old naturalists, the Wels swallows not only young children but also adults. Thus Smitt quotes Gesner (1558) that in the stomach of a Sheatfish were found a human head and a right hand with two gold rings. Then Pallas (1831, p. 82) says that "in some Russian rivers . . . this very voracious fish does not fear to lay hold of the feet and legs of adult swimmers." And Cuvier and Valenciennes quote Ker. János Grossinger (1793) that in Hungary two young girls, who had gone to fetch water (from the river Danube?) were devoured by great catfish. Grossinger also stated that he had been told that on the frontiers of [European] Turkey a fisherman took a great Wels whose stomach contained the body of a woman, together with a gold ring and a purse full of money.

But the reader is asking, "How accurate are these reports and how much of them is to be believed?" Before attempting to answer, it will be well to recall some corroboratory statements about similar feeding habits of the huge catfishes of the Amazon and its tributaries.

These are contained in my article on "The Giant Freshwater Fishes of South America"

(1943). Therein I quoted Dr. J. D. Haseman, whose field studies of South American fresh-water fishes are notable. In the Guapore branch of the Mamore River a great catfish called Pirahyba was captured. Of it, Haseman wrote me: "The mouth was amply large enough for a small Brazilian to crawl through. Indeed it is claimed that the Pirahyba does swallow swimming natives, but I was unable to confirm this."

In the same article, in corroboration of Haseman, I quoted from Theodore Roosevelt's *Through the Brazilian Wilderness* (1914) as follows:

> The huge catfish which the men had caught . . . with the usual enormous head, out of all proportion to the body, and the enormous mouth, out of all proportion to the head. Such fish . . . swallow very large prey. This one contained the nearly digested remains of a monkey, and once engulfed in that yawning cavern there was no escape. . . . Our Brazilian friends told us that in the lower Madeira and in that part of the Amazon near its mouth, there is a still larger catfish which in similar fashion occasionally makes a prey of man.

Roosevelt then quoted the expedition's doctor that at the mouth of the Madeira he had seen a huge catfish measuring 9 feet long. The doctor reported that swimmers fear these great catfish more than they do the big crocodiles. Further, Colonel Rondon (Roosevelt's guide and mentor) told him that in the lower Madeira the villagers so fear the Piraiba (another spelling of Pirahyba) that they build stockades around their bathing places to prevent its attacks.

Let us now return to the Wels and its alleged human victims and attempt critically to evaluate the accounts quoted above. They begin with two of the founders of ichthyology, the great encyclopedists Gesner (1558) and Aldrovandi (1613). They continue with Richter (1754) and Pallas (1813). Some of these allegations were summarized by Bloch (1785), who quotes author, book, and page. Others are given by Cuvier and Valenciennes (1839), who also document their statements. It would seem that these men all believed in the truthfulness of the accounts, and apparently so did Heckel and Kner (1858) and Smitt (1895).

"Where there is so much smoke, there must be some fire." These very similar accounts range from 1558 to 1858 and 1895. They are made by reputable men, who either had direct knowledge of these matters or who had reason to give credence to the men quoted and to their accounts. And it should be noted that most of these men presumably did not know what the others had written. Considering matters thus, one cannot say "Preposterous!" and reject these accounts.

In my own observations of the behavior of fishes, I have seen them do many unusual and extraordinary things. And as a bibliographer of fish literature and a long-time reader of books of exploration and travel, I have learned so much of the unusual and unexpected things that fishes do that I am prepared for almost any behavior not physically impossible. This is certainly true of their feeding habits. A hungry fish will ingest almost anything that gives promise of sustenance. Certainly these great specimens of *Silurus glanis* have mouths and gullets large enough to swallow small children, and impelled by hunger they might seize and ingest such children playing in the water near shore. But, as suggested by Smitt (1895), they are much more likely to prey on corpses, particularly of children, which in some way —by drowning or some other accident—have gotten into rivers.

With regard to the alleged ingestion of adults, the situation and conclusions are very different. If some scientific man could dissect a really big Wels and measure its stomach, he could get some idea of what a big Glanis could swallow. But no one knows how large the stomach of a great Sheatfish is. Smitt (1895, p. 698) states that "the length of the abdominal cavity measures in young sheatfish about one-third and in older ones about one-fourth of that of the body." Figure 1 shows that the Wels has a large abdomen. But that abdomen contains not only Sheatfish about one-third and in older ones testine, a large-lobed liver, a gall bladder, a pancreas, a spleen, an air bladder, and finally the excretory and reproductive organs. Smitt's statement, taken from his description of the abdomen of a Wels, needs to be clarified by actual measurements. What we need are measurements, in figures, of the mouth (the gape, both right-left and ver-

tical), of the gullet, and then of a dissected stomach of a big Wels (say 10 feet long) and an estimate of its volumetric capacity.

But I do not believe that a 9- or 10-foot or even larger Glanis could swallow and contain in its stomach the body of a large boy, and certainly not that of a grown man or woman, as has been alleged. It would seem that such accounts might have originated in the finding in a fish's stomach of parts of a dismembered floating human body—a head, as in Gesner's account, or an arm or a leg. Furthermore, if the body had been in water long enough for decomposition and disintegration to have softened the tissues, it would be entirely possible for two or more of these great catfish to have seized arms and legs and to have brought about dismembering of the body and the swallowing of its parts.

So I have seen powerful 10- and 12-foot tiger sharks lay hold of the legs of a dead horse floating out with the tide just off the Mallory Line dock at Key West, Florida. Rearing and bucking, each trying vainly to tear a leg off, they drifted out of sight, carrying the horse along. Their attempts were in vain, because decomposition had not yet set in, and the hide and muscles and tendons were still intact. But indicative of what these powerful sharks can do when decomposition and disintegration have loosened hide, muscles, tendons, and cartilage, the following evidence may be adduced:

In the stomach of one big tiger shark at Key West, Florida, I found a horse's skull, with the soft tissues all digested, and the skull so softened by the hydrochloric acid of the shark's digestive juice that the bones could be almost crushed in the hand. From another stomach I extracted the dehorned skull of a cow with the flesh gone and the bones softened. In the stomach of a third I found the leg bones of a horse and separate from one of these a hoof with its iron shoe, which I now have in my office in the Museum. Both the heads and legs were torn from the animals' bodies when decomposition had made it possible for the sharks to dismember the bodies. When men's arms, legs, and even heads are found in the stomachs of large sharks, I strongly suspect that they were not bitten or torn from live or just-dead men but from the corpses of drowned men after disintegration of the tissues had taken place.

If a large Sheatfish could swallow a duck or a poodle dog, as alleged, I can see no physical reason why such a great fish could not have swallowed a small child. And if sharks can tear apart and swallow the objects I have obtained by dissection of their stomachs, I can see no reason to doubt that a large Wels, with or without the help of a fellow fish, could have dismembered the decomposing body of a drowned man, have swallowed a part of it, and thus have given rise to the allegations recounted above. But to prove the point one way or another we should have dissections of the stomachs of a considerable number of large sheatfish and careful examinations of their contents. In this way only can the matter be cleared up.

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CAUSATION, CHANCE, DETERMINISM, AND FREEDOM IN NATURE*

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EMPIRICAL science concerns itself with the beliefs and propositions that are verified by reference to "existential" objects and events. If a proposition be empirical, it must designate some property or invariant relation exemplified in some class or domain of things and events, or else imply other propositions that terminate in such reference. In empirical science, with which we are here alone concerned, three fundamental types of invariant relations are distinguishable: (1) *numerical equations;* (2) *certain comprehensive theories or laws;* and (3) *causal relations.*

(1) Many invariant relations are expressed in terms of numerical equations. The principle of the lever states that equilibrium is obtained when the two weights vary inversely as their distances from the fulcrum; within certain limits, the volume of a gas varies inversely as the pressure exerted upon it. Invariant relations of this type are independent of the temporal order of events and are not ordinarily regarded as causal in character. They merely assert that the measured elements stand to one another in a certain invariant relation.

(2) A second type of order in nature is exemplified in such comprehensive theories as the general theory of relativity and the kinetic theory of matter. As a rule, theories of this type are not directly verifiable; i.e., some or all of the events between which the invariant relation is asserted to hold are not directly observable. Thus the electrons, their motions and collisions, the energy of their several motions, and the like, cannot be observed.

The function of such comprehensive theories is to relate many numerical and other laws previously assumed to be isolated from, or independent of, one another. Thus both light and gravitational phenomena are now subsumable under general relativity theory; thus Kepler's laws of planetary motion and the acceleration of moving bodies within the earth's gravitational field are both derivable by logical means from the law of gravitation.

(3) A third fundamental type of order in nature is the causal. "The firing of a certain gun was the cause of the man's death" and "The movement of some icecap in the past was the cause of the present morainal lake on the mountain side" are instances of causal order. Causal order is assumed to involve a temporal sequence in the occurrence of events; indeed, it is commonly believed that it is their temporal order that enables us to distinguish which events are causes and which effects.

HISTORICALLY, three concepts of causality may be distinguished: (1) the *primitive,* or *anthropomorphic;* (2) the *empirical,* or *positivistic;* and (3) the *mathematical,* or *equational.*

(1) The primitive concept of causality is an interpretation of natural changes in terms analogous to those employed in human behavior. Thus, "X pushes his office chair and causes it to move" is supposed to express a causal relation, because there is an agent X who exerts a force against the chair and causes it to move. Similarly, the past icecap is said to be the cause of the morainal lake because there was an icecap which produced the depression in which the water formed by the melting snow and ice was impounded. To common sense, all changes require "causes" to explain them, and when found these causes are construed as agents producing the change.

The definitive characteristic of the primitive concept of causality is the idea of production. The origin of this idea would seem to be the consciousness in the individual of his power to produce effects, for we have the experience of exerting a force and thereby

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producing an effect. The primitive concept implies *efficiency, creation, production.*

Moreover, the primitive concept of causation assumes contact between "agent" and "patient," between a cause and its effect. Yet many causes seem spatially to be separated from their effects. How, then, may such actions be explained?

In accounting for actions at a distance, three explanations have been proferred: (a) The causal action was once interpreted as pure action at a distance. A century or more ago, gravitational, electrical, and magnetic actions were in fact so described. However, the concept of pure action at a distance is now wholly abandoned. (b) Distant action has also been construed as continuous action after transport. Thus a projectile fired from a gun traverses the intervening space and sinks a ship five miles away. (c) The favorite interpretation of action at a distance has been to explain it as action through a continuous medium. Thus ether waves within the range of 365 to 800 millionths of a millimeter in length originating in a distant star traverse the intervening ether and at some later time produce an electrochemical change in the retina of an observer here on the earth.

(2) Unfortunately, it is extremely difficult, if not indeed impossible, to make clear what is meant by causes "producing" their effects. Upon reflection, all we seem to observe in all the alleged instances of causation is some invariant relation of occurrence between two or more changes or events. Hence it is the *invariance in occurrence* that we identify with the "empirical," or "positivistic," concept of causation. Moreover, the empirical definition of causation is logically simpler than, and hence for purposes of systematic science preferable to, the primitive concept in at least three respects: (a) it frees causation of all anthropomorphic elements; (b) causal relations need not be restricted to mechanical action between cause and effect by means of direct contact or through some connecting medium postulated for the purpose; and (c) temporal order or asymmetry ceases to be an essential characteristic of the causal relation. These last two points require elaboration.

The belief that all causation is essentially mechanical in character is very widespread. A corollary of this belief is the assumption that the cause must be equal to, or greater than, the effect. Thus if a moving billiard ball A collides with, and imparts to, a second ball B a given amount of kinetic energy, ball A must possess a kinetic energy equal to, or greater than, that imparted to B. Granted that this principle holds within the domain of perceptible bodies, it by no means follows that mechanical causation of this "push and pull" type is descriptive of causal interaction between electrons or other constituents of matter, especially when these latter are no longer conceived as spatial in character. But if the ultimate constituents of matter be stripped of all properties save energy or mass, a trend already strong in submicroscopic physics, the concept of one solid, extended body imparting motion to another solid, extended body is clearly inadequate as a generic definition of physical causation. The rise of relativity and quantum mechanics, in which physical causation is expressed in terms of differential equations of mathematics, even now bids fair to supplant the Newtonian mechanics based on the concept of matter as ultimately spatial in character.

To common sense, as well as to many scientists, temporal order is also deemed a necessary attribute of the causal relation. Yet, as we have seen, it is possible to define causation without reference to temporal order. For time is not the precondition of the occurrence of events; rather, time is a function of events. Time does not generate events; rather, events generate time. Without events there is no time; events can be defined without reference to time, whereas time cannot be defined without reference to events. Time is one type of asymmetrical order among events. That the psychological awareness of time is a function of discrimination within or among events, or else the awareness of succession between events, seems now well established. That time is a function of events is even more obvious if we turn to chronometer time, for a chronometer, whether a clock or successive transits of some celestial body across the meridian, is but a name we give to some regular

and recurrent set of events with which all other changes or events may be correlated.

That time is in no necessary sense the essence of the causal relation may be seen from another side. While in many instances causal order does exhibit a temporal sequence or asymmetry, from whence it is commonly supposed that the "cause" must precede the "effect" in time, yet, in many other instances of causation the temporal span is absent or else impossible of accurate specification. Hence it is the invariance in occurrence, not the temporal sequence, that constitutes the essence of the empirical concept of causality. Two events or specific sets of events are causally related when neither occurs without the other.

Yet few or no cases of the actual occurrence of two events or sets of events are such that neither occurs without the other. While a certain degree of frost seems always to be followed by the death of the flowers in the garden, flowers often die in the absence of frost. On the other hand, while yellow fever seems always to be contracted through the agency of certain microorganisms deposited in the blood stream of the victim through the bite of a certain mosquito, many persons are bitten by infected mosquitoes who do not contract the disease. In light of this, we may, following Cohen and Nagel, define causation in a somewhat less rigid form. "By the *cause* of some *effect* we shall understand, therefore, some appropriate factor invariably related to the effect. If A *has diphtheria at time t* is an effect, we shall understand by its cause a certain change *C*, such that the following holds. If *C* takes place, then *A* will have diphtheria at time *t*, and if *C* does not take place, *A* will not have diphtheria at time *t;* and this is true for all values of *A*, *C*, and *t*, where *A* is an individual of a certain type, *C* an event of a certain type, and *t* the time."[1]

Moreover, causal relations may be either unilinear or multilinear; indeed, in any given situation they are *both* unilinear *and* multilinear. In other words, precisely which events are inferred to be causally related is in part determined by the context of factors within which the relation is considered. Suppose that at a given time *t* I depart from my office and turn down a certain walk and pass under a given tree, and that at time *u* a given leaf detaches itself from a certain twig overhead and lodges on the brim of my hat. Which of these conditions is the cause of the last-named event?

Depending on the context of factors within which the question is considered, any one of them, or any portion of them, or even all of them may be described as the cause or causes of the event. For, had my purpose of the moment been otherwise, had my teaching schedule called for a class meeting at times *t* or *u*, had no gust of wind arisen then, had the walk been laid out elsewhere, had not some condition of temperature, sap, parasite or other factor affected leaf and twig, in all probability the particular leaf in question would not have lodged on the brim of my hat. Since this particular event was contingent on a certain conjunction of many other events, any one of these latter may be exhibited as a cause of the event. Indeed, we may say that any one of them is not only *a* but *the* cause of the event, for, assuming all the causal conditions of the event to remain constant save one, then the presence or absence of that one will determine the occurrence or nonoccurrence of the event. No event can be the cause of another event in the absence of which the effect occurs.

The determination of the particular events between which the causal relation is affirmed to hold is also conditioned by the purpose of the inquirer. The answer to the question, "*Who* killed Abraham Lincoln?" must be of the form, "A certain person, John Wilkes Booth, was the cause of his death." On the other hand, the question, "*What* killed Mr. Lincoln?" must be answered in terms of the type of causal order of which we are in search. One answer might be, "A certain pistol was the cause of his death." Another might be, "Loss of blood was the cause of his death." Moreover, what is an adequate answer to one question will not in general be adequate to another question.

But if causal relations are conditioned by the context of factors within which the rela-

[1] Cohen, M. R., and Nagel, E. K., *An Introduction to Logic and Scientific Method* (New York, N. Y., Harcourt, Brace and Co., 1934), p. 248.

tion is considered, as well as by the purpose of the inquirer, so also, though in a different manner, are the "events" between which the relation is affirmed to hold. For events, in so far as significant for scientific purposes, are "constructs;" they gain their specificity, not because of any natural cleavages or boundaries inherent in them independent of our experience of them, but only because they represent some one or another aspect of the totality of process or change singled out in attention for purposes of understanding and control. Both the *nature* of the event, as well as the *limits* that mark each event from every other, are relative to, or are in part determined by, both the act of perception and the purpose of him who perceives or thinks about them.

That all events, at least in their qualitative phase, are functions of the perceptual process of him who perceives them is easily shown. Consider, for example, the qualitative characters of the series of events that we call a sunset. That the particular colors perceived own no existence apart from some perceiver is evident the moment we discover that the colors perceived are the result of a complex process of interaction between the perceiving organism with its brain, cortex, optic nerve, retina, cones, etc., on the one hand, and certain ether waves or moving particles external to the organism on the other hand. That the colors perceived are "psychological," not "physical," is now generally recognized by natural science. The color perceived is not the physical stimulus to the perception of itself; it is a "sensory" quality, not an attribute of external nature as such. In like manner it can be shown that the other qualitative characters of events are in similar case. Yet this does not imply that things exist and events occur only in the mind of him who perceives or thinks about them; it merely asserts that the initial data of empirical science—the things, events, and relations between them observed in sense perception—are relative to the observer and are functions of his interactions with certain forces in the world of physical nature objective to him.

There is, however, another sense in which the character of events is determined by the inquirer. We refer to the limits or boundaries ascribed to events. Here it is the purpose of the inquirer that so determines them. Consider the event or, more accurately, the series of events, known as the Battle of Stalingrad. When did the battle begin; when did it end? Did it begin when the first German entered the city limits of Stalingrad? Or, if not then, in the fighting on the approaches to the city, and, if so, at what remove from the city? Did it end with the formal surrender of von Paulus, or only when the last German was safely rounded up, or in some other event? Obviously, any answer to either question is arbitrary, arbitrary because dictated by the purpose of the inquirer.

Nor is the case otherwise if we consider the battle as a series of more circumscribed events. However simple or complex any event be taken to be, the discrimination of it as an event is always dictated by the purpose of the inquirer: This is as true of the motion of a single electron through one millimeter of space as it is of the Battle of Stalingrad. In short, scientific inquiry is no mere staring at the continuum of flux or change that is nature, in which a given event magically detaches itself from the flux and thrusts itself before attention to the exclusion of all other events; rather, scientific inquiry is *purposive,* which is but to say that only such aspects of nature's processes are sought out or are instituted by us as are relevant to the purpose of some inquirer.

(3) We turn to the *functional* concept of causality, in which the statement of order in terms of the familiar language of causation is coming slowly but surely to be replaced by mathematical functions or equations. While physicists still speak of "bodies acted on by forces," and the like, yet, when they express the changes described by this phrase in quantitative form, we find actions replaced by changes in certain physical coordinates or parameters, and "force" denotes that aspect of the change that varies as Ma, the product of the mass by the acceleration. While the tendency to reify or hypostatize such expressions as Ma is still strong (a source of much error and mystification in science), the whole tendency of modern

experimental and mathematical physics is to eliminate the concept of matter as an ultimate substance, and to find the elements of permanence among events in the mathematical relations.

The translation of causal relations into the language of mathematical equations may be illustrated in the fields of classical dynamics and quantum physics. A typical example of the causal process in classical dynamics is the motion of a particle projected vertically upward in the earth's gravitational field. If we know the mass of the particle, its distance from the surface of the earth, and the time and direction of its movement as measured from a fixed origin, then the causal process can readily be translated into a differential equation of the second order, in which time again enters into the determination of the concept. Time reappears, however, not as a perceptual relation, but as an independent variable expressed in arithmetic form. And, since all the determining variables are expressible in terms of number, the idea of efficiency or production is entirely eliminated in description of the process; indeed, the changes described by this equation are no longer stated in the familiar language of causation. Yet the equation contains all that is essential to a causal description of the process, for, given the position and state of motion of the particle at any instant, we can calculate its position and state of motion at any earlier or later time. In short, the differential equation gives a detailed causal analysis of the process.

We now give an illustration of the formulation of causality in terms of the concepts of quantum physics. Quantum physics deals with the submicroscopic, or elementary, processes in nature. The classical kinetic theory assumed determinism for all elementary processes; that is, the invariant causal laws of macroscopic physics were assumed to hold within the submicroscopic realm. Since, however, the initial conditions of an atomic system are not determinable, a statistical causality is now assumed to hold for elementary processes, and elementary quantum processes are expressed by a probability function. And, if the initial values of the probability function are known throughout a region and if the values in the surrounding region are controlled during some interval of time, then the probability function is determined throughout the region during that interval of time.

The same sequence of values of a probability function is determined if the same relative initial conditions and values are given, or are assumed to be given, for some other region at some other time. Yet this does not imply that the totality of processes that condition any event exactly repeat themselves in nature; indeed, this seems possible, and then but approximately so, only under laboratory conditions. But it does assume, as does all scientific investigation, that if nature's processes did actually duplicate themselves, then the same set of conditions would always yield the same results. Thus the mode of determination of the probability function expresses the basic principle of causation.

An illustration will aid in clarification of these statements. Suppose we take one gram of uranium I, the time of whose half-decay is 4.5×10^9 years. If a be the number of atoms in one gram of the substance, then the number of atoms of it that will disintegrate within the time specified is $\frac{1}{2}\ a$.

Unfortunately, just as life insurance mortality tables enable us to make no prediction as regards the time when any particular individual will die, the probability function $\frac{1}{2}\ a$ predicts nothing as regards the time when any particular atom of uranium I will disintegrate. Just as we are unable to isolate all the factors that condition longevity, much less assign numerical value to each of them, we cannot isolate and measure all the factors that determine the exact time of decay of a given atom of uranium I. As regards the exact time of their decay, the sole prediction we can make about atoms of uranium I is that, given some finite number of them, a certain proportion of this number of them will disintegrate within some specified time.

Thus in the functional concept of causality we come full circle. Or, in terms of the three principles of order distinguished at the outset, causal order becomes one with mathematical equations, and causality ceases

to be a unique and independent type of order in nature, a long stride in the direction of greater simplicity in systematic science. Yet this does not imply that we should dispense with the common-sense concept of causation; indeed, so long as men are concerned with control of nature's processes, the concept is both useful and descriptively true.

In any case, it is in this functional sense, more than in any other, that causation identifies itself with the principle of intelligibility of nature. In truth, that nature is an ordered cosmos, or that nature is intelligible, is the basic postulate of science. While the evidence in support of this postulate is as yet fragmentary and far from conclusive, the rapid extension of experimental methods to domains of natural events hitherto unexplored supplies cumulative evidence of its truth.

That some measure of faith is a necessary ingredient of all beliefs in empirical science is easily shown. Suppose we affirm that all fish swim or that the law of gravitation holds true throughout the Universe. Granted that, of all the fish thus far observed, all do swim; granted that all relevant observations and experiments thus far made confirm the universal truth of gravitation. Yet in any literal sense these observations "prove" nothing: i.e., all inferences from "some to all" are logically fallacious. Since scientific hypotheses and laws always ascribe some property or relation to *all* members or instances of some class of things or events, and since we must always do this by the technique of sampling, the propositions, "There are some fish which do not swim," and "There are domains in the Universe wherein the law of gravity does not hold true," must always retain *some* probability value, however slight. In sum, empirical science cannot demonstrate the nonexistence of negative instances, save only by the artificial device of definitional exclusion.

WHAT, now, is "chance;" what does the term connote, and of what is it descriptive? "Chance" possesses a number of meanings, four of which deserve mention.

(1) A first meaning of "chance" may be dismissed with brief comment. Here, chance is identified with the feeling of perplexity and uncertainty incidental to indecision when some situation demanding action is viewed prospectively, or else with the sense of indeterminateness or uncertainty resulting from ignorance of the causal nexus of events when viewed apart from human choice and action. In this the "subjective" sense of the term, chance signifies only our ignorance of the causal order of events. And, it need scarcely be added, ignorance is evidence only of ignorance.

(2) A second meaning of "chance" may be illustrated by the question, "What is the chance of obtaining thirteen spades in a given hand at bridge?" Here, chance is identified with probability value. Yet to affirm that the probability of the occurrence of this event is $(\frac{1}{4})^{13}$ is to make no prediction as regards the specific character of the hand in question, it merely asserts that, in the ideal case of an infinite number of hands at bridge, the hand in question will occur with a frequency expressed by the fraction. Moreover, the assignment of mathematical value to probability functions of this type is possible only if three conditions are fulfilled: (a) the alternative events must all be known or be assumed to be known; (b) the alternative events must be mutually exclusive of one another or be assumed to be so; and (c) the alternative events must be, or be assumed to be, equally probable.

In contrast to this mathematical, postulational, or a priori technique, is the empirical or experiential method of determining probability values. What, for example, is the probability of getting a head on a single toss of a single penny? In one experiment, in which a single penny was tossed 10,000 times, heads turned uppermost 5,258 times and tails 4,742 times. And, we may suppose that, as the number of trials increased, the proportion of heads that will turn uppermost will more closely approximate $\frac{1}{2}$. Thus the determination of probability value based on assumptions seems to be verifiable by appeal to experience or to work well in practice.

In any case, and the only point we need make here, the ascription of some law of frequency to the occurrence of an event is no evidence of indeterminism in nature, but,

rather, is itself a kind of order in the occurrence of events which makes possible prediction of the frequency of their occurrence.

(3) "Chance" also denotes zero correlation or absence of concomitant variations between two or more sets of events. Suppose the correlation between variations in sunspots and fluctuations in the price of men's hats in Kalamazoo is zero. Yet the absence of positive or negative correlations between them in no way implies that either set of events is uncaused, much less that each is no part of some determinate order of events; it merely suggests that the two sets are not causally related. For causal order is determinate; any event is not causally related to all other events, but in any significant sense only to certain ones of them.

(4) "Chance" also denotes *objective* chance. By objective chance we mean, not the frequency with which some event may be predicted to occur, nor yet the vague sense of possibility incident to ignorance of the causal order of events, nor yet the absence of correlations between events, but the ascription of indeterminism to certain events apart from all subjective or other factors. Indeterminism, however, does not imply that events deploy themselves *in vacuo*, but, rather, affirms that certain events have no determinate causes, or that, given the determinate causal conditions $a, b, c, \ldots, n$, any one of an indeterminate number of effects $x, y, z, \ldots, n$, may occur. Needless to add, scientific observation and experiment disclose no evidence in support of indeterminism in *rerum natura*.

WE TURN, lastly, to the controversy over freedom and determinism in nature.[2] Are all events wholly determined, or are at least some of them at times and places indeterminate or free?

Historically, three meanings of "freedom" may be distinguished: (1) indeterminism, or objective chance; (2) free will; and (3) self-determinism. Four meanings of determinism also come to light; (1) necessity; (2) the determination of desire and will by causes antecedent to them; (3) coercions and compulsions exerted by environment upon an agent in opposition to its will; and (4) causal or scientific determinism.

(1) Is nature, at least at times and places, indeterminate? One well-known writer asserts that "a world that is at times and points indeterminate enough to evoke deliberation and choice is a world in which will is free" because "will and choice are determining factors." Yet to affirm that deliberation and choice are determining factors is no evidence of indeterminism in nature; at most this but shifts the locus of causal determinism to human deliberation and choice. Natural law is not abrogated when employed as means to human ends. Indeed, it is only in virtue of determinism in nature that deliberation and choice exert causal control over things and events. Yet this is but to affirm that the course of events may be controlled in such a way as to fulfill our desires and purposes. In short, cognitive inquiry is instrumental to supplanting ignorance and impotence with knowledge and control, the very success of which implies determinism, not indeterminism, in nature. By determinism we mean that, given any event A, or determinate set of events $a, b, c, \ldots, n$, only one event or determinate set of events r will occur.

Nor, as we have seen, does Heisenberg's "principle of indeterminacy" afford evidence of indeterminism in nature. Granted that, of a given number of radium atoms, we can predict what proportion of them will disintegrate within some specified time; granted that this type of prediction is purely statistical in reference and enables us to predict nothing as regards the time when any particular radium atom will disintegrate. Yet it is far more probable that our inability to predict that future behavior of a given atom is due, not to actual indeterminism in its behavior, but rather to our inability to specify its age, the physical milieu in which it exists, etc., or, in general, to our failure to devise experimental operations which isolate, measure, and control the course of its interactions with other things. This, however, is but confession of ignorance,

[2] This part of my paper appeared in somewhat altered and more detailed form, under the title of "Freedom in Determinism," in *The Journal of Philosophy*, Vol. 39, 1942, pp. 520–527.

not evidence of indeterminism, in the behavior of individual radium atoms and their constituents.

While there is no conclusive proof that nature is not at times and places indeterminate, and while some measure of faith must always sustain us in denial of indeterminacy, yet, that all events, both physical and otherwise, have natural causes and natural effects conformable to invariant law, seems now so highly probable that the burden of proof rests upon him who would deny it. If this be so, as we believe it to be, then freedom must be found in a world of things and events *all* of which are strictly determined and hence cannot be identified with indeterminism or objective chance.

(2) The ascription of freedom to the "will," or the identification of freedom with the causal agency exerted by desire and volition, likewise points to the truth of determinism. That these processes do occur, if only in the behavior of living organisms, is a fact. But as mere facts in isolation they are neither free nor determined. For both freedom and determinism are assertible only in terms of relations of occurrence between things and events. Hence the question is not whether or no desires or volitions occur, but whether, in their occurrence, they have natural causes and natural effects conformable to invariant law. That all "physical" events are thus describable is the basic postulate of natural science. That all "psychological" events, desires and volitions included, are likewise both determined and determining factors is the basic postulate of psychology as natural science and may now, we believe, be accepted as highly probable.

More specifically, any event may be viewed from any or all of three points of view: *retrospectively,* in relation to events that precede it; in *simultaneous occurrence* with other events; and *prospectively,* in relation to events that follow it. Hence the basic question is whether the relation of occurrence between events is invariant or chance in character.

(a) Viewed retrospectively, all events seem to have causes; indeed, experience discloses no events that are uncaused. The difficulty is not whether or no events have determinate causes; it is the determination of what precisely their causes are. Observation and experiment alone can supply the answer. By "observation" we mean, not the mere parade of events before awareness, but "scientific" or highly refined and controlled observation, in which tested operations are employed in search of evidence in verification of some hypothesis. In this, the one sense alone in which it is cognitively reliable, observation does in fact disclose the specific causal antecedents of an ever-increasing number of events, in consequence of which the hypothesis of indeterminism becomes increasingly less probable. Moreover, when we turn to events amenable to experimental control, it is discovered that *all* such events have definite and determinate causes. Indeed, we believe that those who "argue" most vigorously against scientific determinism will be found actually to oppose, not determinism as such, but chiefly or only the ascription to certain events of causes which, in virtue of emotive, cultural or other "Idols of the Tribe," are felt to be repugnant.

(b) Simultaneous events are also discovered to be causally related in specific ways. Events are simultaneous when no temporal difference can be distinguished between them. When two simultaneous events or sets of events occur in such wise that the occurrence of either is always accompanied by the occurrence of the other, we have grounds for inferring or at least suspecting causal relations between them. When, in addition, variations in one are observed to be accompanied by variations in the other, we have further and, we believe, more conclusive evidence of causal relations between them. While simultaneity in occurrence and concomitance in variations between events do not of themselves "prove" the existence of causal relations, much less distinguish which are causes and which effects (a distinction which can be made only by appeal to the body of invariant relations already independently established), they nevertheless constitute added evidence of the truth of determinism.

(c) Viewed prospectively, a given event or set of events seems always to be followed by another specific event or set of events.

This is always true of events when experimentally controlled; moreover, it is as true of "psychological" as of "physical" events. With the rapid extension of experimental methods to an ever-increasing number of events in all domains of existence, it becomes increasingly less probable that there occur in nature events whose effects are indeterminate.

It may, of course, be objected that the evidence now available in support of universal determinism in nature is as yet too fragmentary to enforce conviction of its truth. True, nature is under no compulsion to conform to any kind of order or invariant law, deterministic or otherwise. Nevertheless, all the evidence we have points to the truth of determinism; moreover, as experimental methods are extended to domains of events hitherto unexplored, a rapidly increasing number of them are discovered to exhibit a determinate order in their occurrence, even though no two events will be found to have precisely the same causal antecedents. In truth, nature exhibits many different kinds of order: We need only recall the famous proposition of Poincaré that every set of phenomena, even though they be infinite in number, cannot only be fully explained by a theory that can be developed, but also by infinitely many different theories.

(3) We come, now, to the identification of freedom with *self*-determinism or *self*-determination. Self-determinism, however, possesses two meanings, and it is important that we distinguish between them. In its broader sense, it denotes the fact or belief that every event has effects such that the nature of the effect is "determined" by the nature of the cause. In this ubiquitous sense, it is, as we have seen, highly probable that all events are self-determining. A variant of this view is the conception that all things act in accordance with their own nature. Yet empirically, the "nature" of a thing is essentially the modes of its interaction with other things; to ascribe a behavioral trait to a thing in isolation is mystical, not empirical. The attribution of some quality of behavioral property to a thing as such would seem to be little more than a convenience of language.

Self-determinism possesses also a narrower meaning. It denotes the fact or belief that "volition" is a dynamic agency operating as a cause in fulfillment of the desires and purposes of a self. It is not merely that volitional acts have definite effects conformable to invariant law, but also that *the effects be such as to constitute the ends willed.* It is in this sense that we would identify freedom with self-determinism, self-fulfillment, or "free will."

The meanings of determinism may now be dismissed with brief comment. One exception alone need be noted: Necessity is not determinism; rather, it is irrelevant to both freedom and determinism. Two meanings of necessity may be discriminated, the sense of compulsion and logical implication. The former is psychological, the latter logical, in character. Determinism is not the feeling of compulsion; it is an hypothesis to be tested by objective evidence. This evidence, we believe, points overwhelmingly to the truth of determinism. Nor is determinism to be confused with logical implication; implication is a purely formal relation between propositions and involves no necessary reference to material facts. One event may lead us to expect another event; one event never necessitates or implies another event.

As regards the other meanings of determinism, they are all true, or at least are so highly probable as to be accepted as true. For, as we have seen, all things and events are the outcome of definite causes; indeed, they are both causes and effects. Moreover, all events amenable to scientific observation and experiment are such that the causal relations discovered between them are found always to be invariant in character. Hence scientific determinism is true, or at least highly probable, and, since this implies that form of determinism which determines only from behind, both of these determinisms are true. As regards the remaining meaning of determinism, a qualification must be made with regard to it. The identification of determinism with frustration of desire and impotence of will does not exclude freedom. For only in case *all* desires were frustrated, *all* volitions impotent, would freedom be an illusion. Yet desires and volitions are in fact neither wholly impotent nor wholly

omnipotent: Some desires and volitions are frustrated while others are not. *Where they are fulfilled there is freedom; where they are frustrated or balked there is unfreedom.*

Freedom, thus, is a property of desire and volition. A living organism is free in the degree to which it fulfills its desires and volitions; it is free *in the degree to which its acts fulfill what it wills.* Will is *both* determined *and* free. As a natural effect of prior causes it is wholly unfree and wholly determined; as a motor-affective act which has determinate effects, at least some of which are desired and willed, it is both determining and free. If it were not a cause it would be wholly unfree. Yet always to have determinate effects does not make it free, for, if this alone were the case, then freedom would be the same as determinism. *Those wills are free that have just those determinate effects that are desired and willed.* This is the empirical meaning of freedom.

Thus it is that there is freedom only in determinism. Freedom is but a special case or species of determinism. Hence there is no antinomy between freedom and determinism. All desires and volitions have both determinate causes and determinate effects; yet, though rigidly determined, some of them are free. Thus there is no issue between freedom and determinism; the issue is solely between freedom and unfreedom, or, possibly, between freedom and indeterminism. But since indeterminism is highly improbable, there seems to remain only the question as to whether desires and volitions are free or unfree.

OUR HOME

This evening with the setting sun
I have walked out to another world
Where to the west the day in leaving
Sends back a summer storm,
Which, letting down long banners,
Rustles toward me over the wide plain;
And fields of gold-brown wheat and green corn
Sweep away to a far purple ridge and a paling azure sky.

Somewhere a last bobolink is singing,
A meadowlark toasts the retiring day once more
And the weeds around hum pleasantly—
Insect universes bedding down;
From the fast advancing cloud bower
Sounds the first thunder, shaken about
And echoed back in a scarlet pheasant's raucous shout;
And a somber hen cowers expectantly on her wondering chicks.

Now the first cool drops spatter the dust
And the sun is disappearing beyond the world,
With upflung splendor embracing the storm like a friend
And making it for me a small and friendly thing;
And in a few moments when it is over,
I hear the Angelus of The Sixth Symphony
Played by the storm, the sunset, the lark, and my heart
And Jupiter announces the night.

—Pvt. T. A. Morrill, Biggs Field, Texas.

THE TROPICAL GRAPE

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For hundreds of years people of the Tropics have tried hopefully, almost desperately, to cultivate the grape. Literally hundreds of domestic varieties and every conceivable method of culture have been tested without success. But tropical varieties had never been planted.

Since climate is a controlling influence in the growth and adaptability of all plants, it is advantageous to consider the grape with respect to its geographical distribution. It is found that all species and domestic varieties, wild and cultivated sorts the world over, fall into three climatic groups: the arid-temperate, the humid-temperate, and the humid-tropical.

The arid-temperate group is composed of those species and domestic varieties that are native to regions having hot, semiarid summers and moist, cool (freezing) winters. The well-known European vineyard kinds (*Vitis vinifera*), which are believed to have originated in the region of the Caspian Sea, are examples. They are the grapes of antiquity and the Bible, so frequently mentioned in all recordings of the ancient past. Just when man first focused his attention on the original wild type or made the initial efforts at domesticating and improving it is beyond the horizon of recorded history. Viticulture was flourishing in the time of Homer and doubtless came into being long before his day.

Hundreds and even thousands of improved varieties of this climatic group are known, a few of the more prominent of which are: Malaga, Emperor, Sultania, Cabernet, Muscat—all pure forms of *vinifera* species.

The humid-temperate group is native to moist, temperate climates where the growing season is short, moist, and warm and the winters are long and cold. All the wild species native to the United States east of the Rocky Mountains and north of central Florida belong to this climatic classification, as do a few others from eastern Asia. Within this group there are many improved, domesticated varieties, largely derived from the species *Vitis labrusca*, which have been developed in North America during the last 150 years. A few sorts, such as Concord, Delaware, Niagara, Beacon, Catawba, and Isabella, are widely grown in the United States east of California and are popularly known as "slip-skin, or American, grapes," in distinction from the meaty, solid-textured European types.

The humid-tropical group comprises the kinds of direct interest here. Although there have been in existence no improved or domesticated grapes of this class, there has been an interesting and valuable assortment of basic wild material. Fourteen or more wild species having humid-tropical adaptabilities are known, and there is a wide and varied range in the horticultural factors involved.

Within the humid-tropical group of wild species there are practically all of the fundamental qualities needed for the development of a new class of improved tropical vineyard kinds. The qualities of rugged health and disease resistance, high productivity, large berries and clusters, and sweet, delicious flavors can be found in one or another of the wild selections.

Since all species of true grape can be intercrossed with little difficulty, there remains but the matter of a methodical apportionment of the desired characters in conformity with specialized *Vitis* genetics until we realize our ultimate goal—fine vineyard grapes for the Tropics and Subtropics.

In the autumn of 1935, in pursuit of a general interest in botany, I became deeply immersed in the taxonomic complexities of the genus *Vitis*. Along the margins of the glades and streams and in the tropical hammocks of southern Florida there were various wild grape species that were faultily described and in some cases unknown in the reference manuals. Coincident with my difficulties in the identification of these wild

Florida species, I became interested in bringing about their horticultural improvement.

In less than a month my project was roughly outlined and under way. The beginning was slow and difficult. To attempt to bring into being a new race of fruiting plants with superior qualities in a climate and on soil where only local wild types could survive to the flowering age is naturally a difficult undertaking. The acquisition of that initial "toe hold" is generally the most difficult part of any such program.

The first six years of work was performed in the tropical climate of extreme southern Florida, south of Miami. Early in 1942 the project was transferred to Mayaguez, Puerto Rico, for a brief period. Then, in 1943, it was moved to the Institute in Costa Rica, where it is now (1944) officially under way.

Procurement of pollen of the finer temperate-zone domestic sorts constituted the hardest problem in the early stages of the effort. Of more than 150 varieties of the best European and American cultivated kinds that were carefully planted in the test vineyard as potential pollen vines, only six or seven lived to reach flowering age and they blossomed too late in the season to be of value.

Without a local production of pollen no breeding work could be done. Because of the earliness of the tropical season and the low viability of grape pollen when stored, it had become obvious that the success or failure of the entire program hinged on the solution of this difficulty. The handicap was ultimately overcome through the development of a technique that rendered it possible and easy to make any cross desired.

Our tests of the many domestic, temperate-climate grapes alongside the wild tropical selections have afforded a valuable opportunity to study the factors responsible for success or failure in tropical viticulture. While the European and North American cultivated sorts struggled under hopeless climatic and disease handicaps, the wild tropical kinds grew and fruited luxuriantly. This fact has lighted the way through the entire effort.

The collection of the best wild vines from river swamps and tropical jungles for use as breeding parents has been an even slower and more difficult task. Yet in many ways it has been something of a colorful adventure.

Many exploration trips afoot through sultry, snake-infested woods and by canoe down treacherous jungle streams have been highlighted with moments of excitement, disappointment, and joy, along with a varied assortment of discomforts and hardships. The quest for the imaginary and ever-elusive ideal wild vine, which the mind's eye might vision as decorating some secluded tree or canyon ledge with a profusion of fine black grapes, has almost led to Eldorado.

I shall not forget the circumstances associated with the discovery of Number 24. The picture of it is most vivid: the lone canoe journey down many miles of treacherous river, flooded by summer rains; the darkening forest, crowding the channel with jungle growth; the colorful leafy streamers of the Calloosa grape that canopied the treetops; then, in the faint light of a setting sun—a glorious sight! In a reclining tree along the east bank there hung beautiful clusters of large black grapes that suggested Concord. They were sweet and good, and I was overjoyed.

The variety Sable is another parent vine taken from the wild which has an interesting history. It has been used perhaps more than any other in the author's latest breeding experiments. This selection is obviously a midway, or hybrid, form between the large Calloosa (*Vitis shuttleworthii*) and the Redshank-Figleaf grape (*V. rufotomentosa-V. smalliana*) and holds its present horticultural significance mostly through a peculiar act of chance.

It had been previously reasoned that wild hybrids with all-round superior qualities could and should exist within a certain region where the range of two superior species overlapped. It was carefully decided just what was wanted and just how good a grape in all respects might be possible from the best blending of the two species. With this goal in mind a determined search was undertaken.

After long days of search, by canoe and afoot, through the river swamps and forests and after inspecting literally thousands of vines, nothing outstanding had been found. True, there were many supposed hybrids but,

strangely, all of those examined seemed to inherit only the very worst qualities of the parent species.

Late in the afternoon of the fourth day, the large blue-green foliage of another of these midway types was observed sprawling over a group of palms and small trees along the left bank of the stream. A somewhat hasty inspection was made, a few moderately large and sweet grapes were tasted, but the vine was adjudged as of no especial value due to an apparent smallness of the fruit clusters. As it was late in the afternoon and there were many miles yet to be navigated through regions where camping for the night would be impossible, the journey was resumed.

Two days later while we were encamped on another stream many miles distant, the realization came that perhaps we had hastily and unfairly appraised this vine. The conviction became so hauntingly acute that we were soon packed and on the way back for another inspection.

A second and more thorough examination revealed typical fruit clusters seven to eight inches long from top berry to bottom. Secluded among the branches there was an occasional bunch that rivaled in size and appearance many of the better vineyard varieties of the temperate climates.

A selection of the wild Calloosa grape, referred to here as "Number 5," has been employed extensively in first-generation crosses. This vine was originally taken from the woods near Miami, Florida, some fifty years ago by J. J. Soar, of that city, and planted in his garden where it is now of immense size and canopies the top of a giant live-oak. Though the large berries of this wild parent plant are of poor edible quality, it has given some very good offspring.

Another wild selection, which we refer to as Number 18 and which belongs to the novel *Vitis gigas* species, has been much used in later crosses. It is healthy, vigorous, and extremely productive of large clusters with medium sized fruit of good quality. Its value was ascertained more by deduction than by the actual observance of characters and then only after sweltering, insect-plagued hours of discomfort. At the time of its discovery just two undersized berries could be found that had escaped the animals and birds, but later performance in the vineyard more than fulfilled every confidence placed upon it. The vine was discovered in southeastern Florida, in general association with such tropical plants as the mango and avocado tree and the coconut palm.

Ten or twelve additional wild selections have been used as parent vines, one of the most important of which is the Number 10. This is a supposed natural hybrid of the large Calloosa grape with the big clustered Redshank-Figleaf type. It has moderately-large sweet fruit, maroon red in color and a vine of great vigor and resistance to disease.

Another, the Number 6, seems to be a natural cross of the Calloosa and the pure Figleaf species. It has medium-large black grapes of moderately good quality, but the vine is a little subject to disease.

THE improved varieties derived from the wild vines of the jungle have afforded an inspiring sight. In the extremely humid tropical climate and waterlogged soils at Mayaguez, Puerto Rico, where all other domestic grapes were impossible to grow, the new kinds have made healthy and luxuriant growths. On the dry and impoverished rocklands of southern Florida their growth has been equally good. Even under the tropical rain-forest conditions (110 inches precipitation annually) at Turrialba, Costa Rica, where grape culture in any sense has been thought impossible, our select-types in the Institute vineyard show beautiful and healthy vines with a normal set of fruit. It might be of interest to add, moreover, that these plants have never in their entire existence known the protection of fungicides.

The first fruit from any of the tropical hybrids was borne in the summer of 1940. One of the most interesting of these is the Fairchild (Number 106), which came from a cross of the wild "agras" of lowland Central America and the large-fruited European variety, Alphonse Lavellee. This unusual grape has a healthy vine with self-fertile flowers and medium-sized (one-half inch) sweet fruit that is several times larger than the berries of the mother species. The va-

riety is completely immune to the difficult grape-leaf rust, which is one of the most destructive grapevine diseases of the Tropics. It has been employed in various secondary crosses.

The new variety Tropico (Number 240) is one of the best descendants of the wild Calloosa grape that has yet borne fruit. It has a strong, productive, disease-free vine with large purple-black berries that are larger and of better quality than the well-known Concord variety of the United States. The fruit is sweet and tender with a thin skin and a delectable, mild, almost *vinifera* flavor. Tropico was originated from Number 5, crossed with a complex hybrid of *V. champini*, *V. labrusca*, and *V. vinifera*.

Marco (Number 3) is a medium-sized, dark-red grape, quite like the variety Delaware. It has in fact a sweet, mild Delaware flavor and an equally melting and tender pulp with small seeds. It is a juicy and truly delicious fruit. The vine is healthy and productive of moderately large compound clusters. Though the berries are rather small (average one-half inch) and a bit seedy for a fresh dessert fruit, they would in all probability make an excellent light-colored wine. The variety has three parts tropical wild parentage and one part temperate cultivated blood. It is a descendant of Number 9 (*V. rufotomentosa*) crossed with a hybrid of the wild Calloosa grape and the northern cultivated variety Niagara.

Another hybrid selection, Wachula (Number 126), has the same parentage as the preceding. It has compact clusters of sweet, medium-sized (one-half inch) black grapes of good mild-vinous flavor. The vine is healthy and very productive.

Crosses between Number 9 and the finer European varieties have given some interesting progeny. One of these, Biscayne (Number 113) has dark red, translucent berries that average one-half to five-eighths inch diameter. They have a sweet, delicious, *vinifera* flavor and are borne in moderately large clusters. The vine is strong, healthy, and productive.

Some combinations between the wild tropical selections and the North American vineyard sorts (humid-temperate group) have given fruit with the Concord, or *labrusca*, taste.

Throughout the course of the project more than 100,000 hybridized grape seeds have been planted. Many thousand seedlings have been grown and hundreds have already borne fruit. Carefully planned secondary crosses, between the best of these newly developed sorts, as again with the better of the wild selections, have been made and are fruiting for the first time. There is every reason to expect some good things from these later crosses.

Plant diseases and other pests are a factor that must always be considered. Under humid, tropical conditions some diseases of the grape are very severe, even fatal, unless the variety possesses special resistance to them. In the development of our novel tropical types we have given much consideration to this matter and have obtained kinds that have health and disease immunity to a remarkable degree. As a result, fungus diseases have never troubled our better selections.

Insects, birds, and animals are occasionally a problem, as in the cooler climates. In Central America the *zompopo*, or leaf cutting ant, may do much damage if not controlled. It is easy to eradicate, however, and would never exist in a well-kept vineyard.

Though it is beyond question that these recent humid-tropical originations are incomparably better adapted to hot, humid climates than are any other of the known grape varieties planted at present in the Tropics, the full limit and range of this superiority must yet be determined. Their value for the manufacture of fine wines and as tropical market grapes presents highly interesting and ever-enlarging possibilities.

Good natural wines (without addition of sugar or alcohol) have been made from a few of the new sorts, but their individual and finer points in this regard must await later investigations.

These new tropical grapes have not as yet been made available to the public. Usually it requires several years after a variety is developed before it can or should be released. Still, we hopefully believe that within a relatively short time it may be possible to make a limited first distribution of a few sorts.

THE RELATION OF NATIONAL PROHIBITION TO MENTAL DISEASE

By CARNEY LANDIS and JANE CUSHMAN
NEW YORK STATE PSYCHIATRIC INSTITUTE AND HOSPITAL

THE Eighteenth Amendment to the Constitution was intended to be a reform and something of a panacea to the social ills of a nation. It was neither proposed nor passed because of scientific curiosity—and yet there has never been either a medical or psychological experiment on so large a scale. Its subjects numbered millions, its duration more than a decade. Essentially, the hypothesis behind it was: can authoritarian control prevent or curb a habit which in many cases becomes a disease? Neither the propaganda nor the political motivation which led to this amendment was aimed at the occasional or moderate drinker. The avowed goal of the legislation was the alcoholic proper, the habitual drinker who fell down on the job if he had one, who filled the city and county jails, who caused his family untold misery, and who made up at least 10 percent of those entering our mental hospitals.

No scientific report of many of the results of this experiment has been made. For a time the corner saloon disappeared, bootlegging became a specialized and highly lucrative means of livelihood, and an indeterminate number of law-abiding citizens locked up their wine cellars. The exact extent of these developments is not known and probably never will be. Speculations have been reported, but speculations without evidence are likely to follow too closely the personal bias of the speculators. There is a body of evidence which does throw light on one aspect of the effect of prohibition, and that is in the published statistical records of state mental hospitals, which, among other things, give in some detail the annual admissions for alcoholic psychoses and for other mental conditions which are more or less related to the intemperate consumption of alcohol. Not only is this information factual rather than theoretical, but also it bears upon one of the main arguments which was advanced in favor of National Prohibition. Because New York is the most populous state in the Union, it has the largest number of hospitalized mental patients in the country. Furthermore, New York has the longest existing system of uniform statistical reporting. For these reasons we have based this inquiry on the reports of New York State since New York's figures provide an adequate representative sample of the effect of National Prohibition on mental disease.

Population statistics can be used to "prove" almost any point any propagandist desires to make. The following evidence, however, is based on simple, legitimate procedures. Most of the *rates* which we have obtained are rates for the particular age or sex in question. This is both important and necessary since there is a vast difference between the rates of alcoholic mental disease for women and men and for the different age groups. For example, if one should include all ages, then the 43 percent of the total population of the nation below age 25, who are but slightly affected by alcohol so far as the mental hospitals are concerned, would make all rates much lower by their inclusion.

We will presently show that the rates of hospitalization for any or all forms of alcoholic mental disease are quite different for men and for women and that this difference holds true in both the age range 20 to 40 years and that 40 to 60 years. If National Prohibition affected the rates of hospitalization, then each trend for alcoholism should be compared to the trend of the rates for *all* first-admissions to mental hospitals for all causes for the four age groups. (Since some individuals are admitted to hospitals once and others have several admissions to their credit, nearly all mental hospital rates are based on first-admissions, meaning that any individual case is counted but once, no matter how often he may have appeared.)[1]

The trend for *all* first-admissions to the

[1] Certain of these data have appeared in an article by the authors in the *Quarterly Journal of Studies on Alcohol*, 5: 527–534, 1945.

New York State mental hospitals, 1913 to 1943, of men aged 20 to 40 and 40 to 60, and women aged 20 to 40 and 40 to 60, per 100,000 of the general population of the state of the same age and sex, is shown in Fig. 1. These trend lines are more or less flat and level until 1930 when they began to increase. Investigations have shown that this increase is due, for the most part, to the erection of additional hospital facilities and, in the case

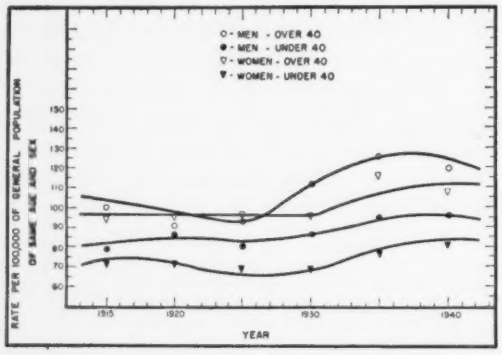

FIG. 1. FOR ALL MENTAL DISEASES
TREND OF RATE OF FIRST ADMISSIONS TO THE MENTAL HOSPITALS OF NEW YORK STATE, 1913–43.

of the older groups, to the steadily growing tendency for the hospitalization of people over 50.

The trend of the rates for the New York State mental hospital admissions for all those reported to be suffering from alcoholic mental disease between 1909 and 1943 has been analyzed by several different investigators. All agree that from 1908 to 1920 there was a relatively constant decrease in the rates of first-admissions to mental hospitals for alcoholic mental disease for both men and women. In so far as entrance to a mental hospital is concerned, in 1920 there were proportionately fewer suffering from an alcoholic psychosis than there were before or have been since. Between 1920 and 1934, however, more and more alcoholics entered the hospitals until the rate reached the level it had known in 1911. Since 1934 the rates have been either static or decreasing.

A scrutiny of the figures given in these annual reports indicated that not only were there different trends with respect to sex but there seemed to be differences with respect to the age at which these alcoholic patients first came to the hospitals. Since age is reported in 5-year interval groups, viz., 20–25 or 45–50, it was possible to work out rates and trends for any five-year age period. On doing this, we found that the trends for rates by age tended to fall into two groups. The trends for ages 20 to 40 and those for 40 to 60 were similar and could be added together. This was true for both men and women.

The trends of the rates for these four groups are shown in Fig. 2. These rates were computed per 100,000 of the general population of the state of the same age and sex. These curves show that each year between 1910 and 1920 proportionately fewer patients in each age group came into the hospitals with alcoholic mental disorders. From 1920 to 1934 there was a gradual increase in rate for each of the groups, which became much less pronounced between 1934 and 1943, actually falling off for the women and for the men aged less than 40. The curves make it clear that the bulk of the variation in the number of alcoholic patients

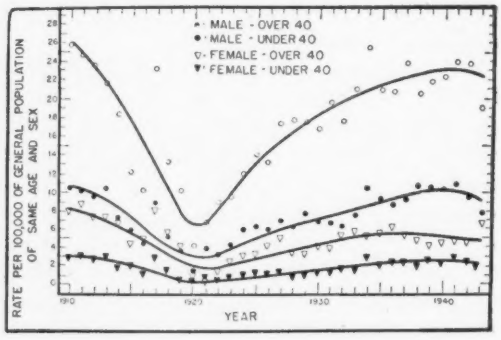

FIG. 2. FOR ALCOHOLIC PSYCHOSES
TREND OF RATE OF FIRST ADMISSIONS TO THE MENTAL HOSPITALS OF NEW YORK STATE, 1909–43.

is attributable to changes in the hospitalization rates of men in middle life, aged 40 to 60. In one sense, this only confirms common knowledge, namely, that it takes 10 to 20 years of heavy overindulgence in alcohol to achieve a mental collapse. Fig. 2 clearly shows that something—possibly National Prohibition—actually did decrease the rates of alcoholic psychoses in men between 40 and 60 for a time; that it had a minor effect on

men aged 20 to 40, and little or no effect on the rates for women.

Compare the trend lines in Fig. 1 and Fig. 2. In Fig. 1 there is no sharp 1910–1920 drop, and the lowest point shown occurs in 1925 or 1926, not in 1920 as in Fig. 2. Now if the pronounced variation in rates shown in Fig. 2 were a true drop—that is, if the patients in question were not only no longer being diagnosed as alcoholic psychoses, but were not coming to the hospitals at all—then there should be a clear reflection of these changes in the trend lines in Fig. 1, since alcoholic psychoses made up about 10 per cent of all first-admission patients between 1910 and 1917 and between 1925 and 1943. (The percentages for the years 1918 to 1924 were, respectively, 5.7, 4.0, 1.9, 2.8, 3.3, 3.6, and 5.5 per cent of all first-admissions.) What happened to the men who did not come to the hospitals in the years between 1918 and 1924—or did they come and did the doctors give them a different diagnosis? This was worth following further.

There are different kinds of mental disorders caused by chronic alcoholism. For the most part these are named in accordance with the particular symptoms displayed. These subvarieties go by the medical names of delirium tremens, alcoholic hallucinosis, Korsakoff's psychosis, chronic alcoholic deterioration, alcoholism without psychosis, etc. When the annual rates, 1912–1943, for the subvarieties among men aged 20 to 60 per 100,000 of the general population were computed, little annual variation in rates was found for the well-defined subgroups, viz., delirium tremens, alcoholic hallucinosis, or Korsakoff's psychosis. The great variation existed in the miscellaneous category. This category is made up of alcoholism without psychosis, alcoholism complicated by other forms of mental disease, and several of the less frequently reported subvarieties. The implication of this point depends on other evidence which we will next consider.

The *intemperate use of alcohol* among *all* first-admission patients of all diagnoses, expressed as a rate per 100,000 of the general population of the same sex between the ages of 20 and 60, is shown in Fig. 3. The groups represented by these curves had a steadily

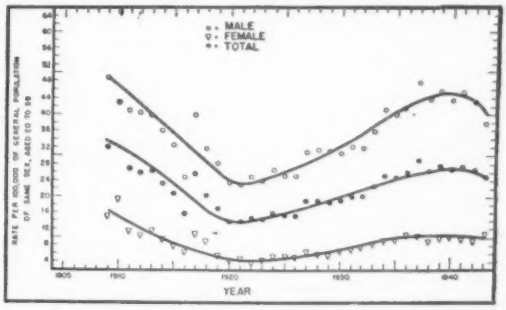

FIG. 3. EXCESSIVE DRINKING ALLEGED
TREND OF RATE OF FIRST ADMISSIONS TO THE MENTAL HOSPITALS OF NEW YORK STATE, 1909–43.

declining rate before 1920, but between 1920 and 1934 their proportion mounted, and after 1934 remained practically static. If, however, Prohibition were prohibiting even partially, then alcohol could not be used intemperately at all.

Since 1889, New York State has published the figures giving the total number of new mental patients admitted to the mental hospitals together with the "cause" or principal precipitating factor as reported by the family, by friends, or by the court. The trend of these figures for alcohol as the principal cause, expressed as a percentage of the number of patients admitted to the hospitals, is shown in Fig. 4. These curves show that the percentage of patients whose illness, according to the relatives or friends, was caused by excessive drinking, decreased between 1889 and 1891; increased 1892 to 1909; decreased 1910 to 1920; increased 1921 to 1934; and has been decreasing again since 1934. The highest percentages were in 1889, 1909, and 1934, and the lowest in 1891, 1920, and 1943. If these curves mean anything, they mean that the use of the explanation that alcohol causes insanity is not a constant excuse. The waves of the trend lines are not associated with the official figures giving the amount of alcoholic beverages sold in the state, with economic conditions, nor, in more than a vague fashion, with Prohibition. As soon as Prohibition went into effect, the use of the explanation, "alcohol, principal precipitating cause," started to increase, but it did not go out of style for several years after prohibition had been repealed.

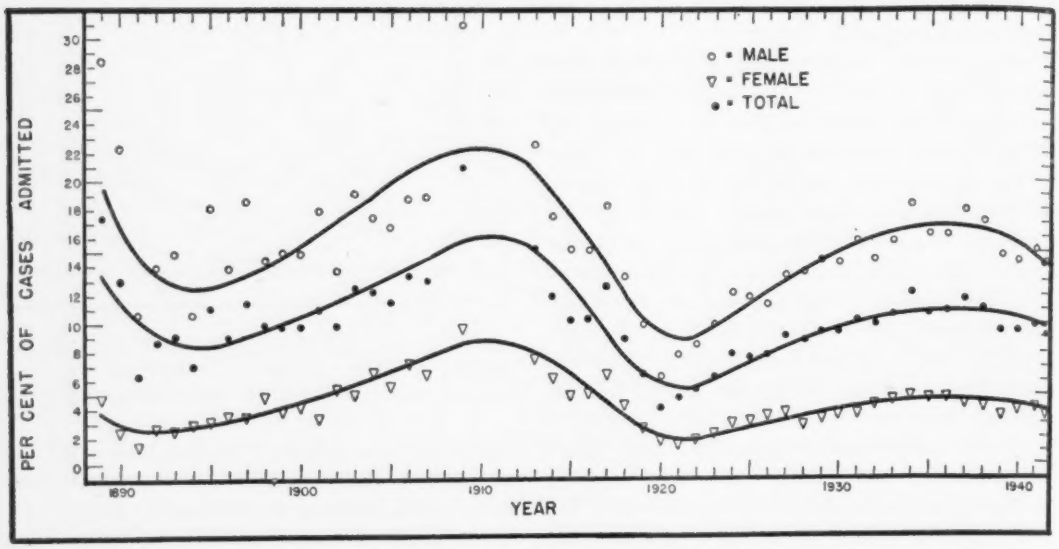

FIG. 4. ALCOHOL SAID TO BE PRIMARILY RESPONSIBLE FOR HOSPITALIZATION
TREND IN PERCENT OF CASES ADMITTED TO THE MENTAL HOSPITALS OF NEW YORK STATE FROM 1889 TO 1942.

The evidence from these trend studies indicates the following:

1. The rates at which persons were admitted to mental hospitals and diagnosed as suffering from an alcoholic psychosis were decreasing between 1909 and 1920, and increasing between 1921 and 1934, after which year they have been relatively constant.

2. The main group who caused this decrease and increase is that of men aged 40 to 60.

3. The variation in rate is chiefly attributable to the "miscellaneous" grouping of alcoholic disorders. Those forms of alcoholic mental disease which are clearly related to alcohol and only to alcohol, show little increase or decrease in rate.

4. Although there was a marked decrease and increase in the rate for alcoholic psychoses between 1910 and 1943, there was little or no reflected change in the rates of admission for all mental disorders. This means that candidates for mental hospitalization presented themselves at their regular rate despite Prohibition, but the hospital physicians saw fewer whose symptoms were complicated by alcoholism.

5. The trend of the rate of admissions to mental hospitals of those who were said to be intemperate in the use of alcohol closely paralleled the trend of those for whom a diagnosis of alcoholic psychosis was made. That is, it decreased from 1909 to 1920, increased 1920 to 1934, and has been level or declining since 1935.

6. The percentage of patients entering mental hospitals for whom it is said that intemperate use of alcohol was the principal cause leading to hospitalization has varied widely between 1889 and the present. Between 1909 and 1920, before Prohibition was effective, there was a constant decrease in this percentage while from 1920 to 1934 there was an increase, though most of this time saw Prohibition legally in effect.

These are facts which justify some conclusions and certain questions. It is a safe conclusion that National Prohibition did not affect the rate at which patients enter mental hospitals. If it did anything, it accelerated the rate.

Alcohol and mental disease bear a complicated relation to each other. Many persons who are experiencing the first symptoms of mental disorder resort to alcohol as a means of drowning their otherwise inexplicable feelings of guilt, anxiety, unreality, disorientation, etc. Others drink because the disease process weakens their previous good judgment, self control, and normal inhibi-

tions. In either case the alcohol may make the symptoms acute—act as the major precipitating cause, and even conceal the true disease when the person first reaches the hospital. In such instances the effect of Prohibition would show up in statistical reports in two ways. First, if there is real difficulty in obtaining alcoholic beverages, then the case will eventually reach the hospital uncomplicated by alcohol. This is the simplest explanation for the immediate decrease in 1919, 1920, and 1921. There was no real decrease in alcoholic psychoses, but there was a decrease in cases in which alcohol was a complicating factor. Second, the physicians who admit the patients to the hospitals and who fill out the statistical report cards were aware of National Prohibition and hence did not "see" so much evidence of alcohol in the patients who came to the hospitals.

The true alcoholic psychoses are achieved after ten to twenty years of steady heavy drinking. If prohibition had affected this sort of mental disease, then the real decrease should have shown itself in a decrease in the rate of chronic alcoholic deterioration and Korsakoff's psychosis cases during the 1930's. There was no such decrease.

We are not interested in reviving the old propaganda battle for and against Prohibition. The facts here put forth must be explained, and if any explanations are to be credited, they must bear on three main questions. First, if temperance propaganda caused the 1910–1920 drop in the rates, what caused the drop back in 1890, before the social pressure resulting in the Eighteenth Amendment had gained effective headway—and why have the rates been decreasing since the late 1930's? Second, why were increasingly more alcoholics admitted to mental hospitals between 1920 and 1934, an era when legal restriction of liquor traffic was part and parcel of the Constitution? And finally, why, when repeal was put through, did the rates proceed to level off or decrease instead of increasing suddenly and mightily?

Certainly no simple cause and effect relation of National Prohibition to mental disease provides a satisfactory answer to any of these. It is safe to say that the argument that legal prohibition of the sale of alcoholic beverages will decrease the number of patients entering mental hospitals was decidedly not substantiated by the experience of New York State mental hospitals—and that, within the limits of our data, the "noble experiment" gave more negative than positive results.

HI HO AND HO HUM

Old Hi Ho first appeared in the SM on page 243 of the March, 1945, issue. His words of wisdom on how to make "everything all right" struck one of our oriental readers as too sanguine. He felt that Old Hi had been paying more attention to Pollyanna than to Confucius and was tempted to reply in a vein that would call for the signature "Ho Hum." Then the editor stepped in and persuaded these two doughty sages to prepare themselves for verbal combat. In keeping with the Christmas season he suggested that they argue the perennial question, "Is there a Santa Claus?" So, Virginia, you will find their thrusts on page 502 of this issue.

As both Hi Ho and Ho Hum are loyal members of the A.A.A.S., their thoughts naturally turned to Santa Claus as a potential contributor to the centennial building fund. Old Hi tries to persuade us that Uncle Sam and Santa Claus are identical (though everyone knows one is tall and thin and the other short and fat). He expects Sam Claus to put a thumping check in the Association's stocking. If you believe Old Hi, Sam Claus will provide for us a building not unlike the modest little structure he erected for "The Nine Old Men." To this daydreaming our other oriental friend says, "Ho Hum." But he really believes in Santa Claus too, not as a mythical head of the body politic but as a "polypus" of 25,000 hard-working scientists. It is not too much to hope that our polypus has white whiskers and a twinkle in his eye, that he knows our great need for a building in which to work for him, and that he will surprise us this Christmas.—ED.

TRANSFORMATION IN SCIENCE*

By J. D. BERNAL, F.R.S.

UNIVERSITY PROFESSOR OF PHYSICS, BIRKBECK COLLEGE, UNIVERSITY OF LONDON

ONE of the paradoxes of the present time is that people may be able to change the world so rapidly that they fail to understand what they are doing. Another is that, while more has been found out at large and in detail about nature and man in the past thirty years than in the whole of history, there is less general appreciation of this knowledge and worse use of it than ever before. This is true partly because modern science has become more complex, but as much because it has become professionalized. Since some people are paid to understand it, why should the rest bother their heads about it? But ignorance of science means a failure to understand the factors underlying the critical events of our time. The history of the last few years should have shown that it is no longer optional, but absolutely necessary, for science to be understood, appreciated, and effectively used.

The tragedy of the present struggle is that the ends for which people are striving—food, work, security, and freedom—are gifts which science has put within reach of all. The resources, the knowledge, and the ability to build a new world are there, but instead we have danger and bloodshed, want and misery. If people could understand at least something of the possibilities which science offers, they would become more reasonably impatient of their present state, and more capable of changing it. For this, science needs to be expounded, and expounded in a new way which emphasizes its relation to this changing world. It is no use any longer attempting to present science as a series of pictures of the beauties or the mysteries of the universe and of nature. People have had enough of that already; it belongs to a time when individual and social security and the general running of society could be taken for granted. Indeed, the public is very justifiably irritated with the idea of the pure scientists' leisurely and secluded search after minute and remote things, when the world all around is being bombed to pieces; especially as airplanes, guns, tanks, and other engines of destruction seem to be the most noticeable products of scientific research.

But in any case the scientists themselves are no longer anxious to present a merely academic picture of a disinterested search after truth combined with a sublime indifference to the results of discoveries. Science has long been much more than this. It has become an integral part of productive industry and agriculture, it maintains health, it is increasingly involved in business administration and government. The methods and ideas of science are the dominant forms of thought and action in our time.

Science is no longer a spare-time occupation of a few dozen gentlemen of leisure, it is the whole-time job of some hundreds of thousands of research workers in nearly every country of the world. Science has become an industry, a small, but key industry. The cost of scientific research is borne directly or indirectly by industrial contributions, and already there are far more scientists working for industry than in universities or independent institutes. The very progress of science itself would be quite impossible without that of industry. The great discoveries of the present century were made possible by the industrial application of nineteenth-century discoveries. Without the mechanical technique or the ready availability of instruments of the chemical and electrical industries, modern physics and chemistry could not exist.

The war has given a terrible urgency to the problem of the proper relation of science to human affairs. It turns out that although science has been used very largely for the development of weapons, it is needed and used almost as much for the problem of preserving the life and health of the military forces and the civil population under the most difficult circumstances. This brings to the foreground the essential function of science, which is to find the means of satisfying

* A radio address, British Broadcasting Company.

the most elementary human needs. What has been seen as a necessity in war, was no less a necessity in peace. If the function of science had been fully realized then, the want and misery which led to the war would have been removed without the need for a struggle that can only waste human resources and destroy the powers of human thought. Modern science was created by the same movement that made capitalism. It is strongly attached to ideas of individual initiative and freedom of thought. However, the result of the combination of scientific technique and capitalist economy has been the creation of national and supernational monopolies, in the growth of which the old individualistic methods of industry have largely disappeared. Modern science, with its expensive equipment, its need for elaborate organization, and its close relation to industry, did not, indeed, even before the war, conform to the liberal idealist picture. Independent scientists had almost disappeared.

The war has already resulted in bringing science in every country in the world, America included, into the orbit of national defence on the basis of organized planning. Liberal scientists have a very natural fear that this will result in the destruction of the spirit that made science possible and in the loss of the ideals of free inquiry and free application. Some are even willing to acquiesce in a situation in which science will be quite a minor and ill-rewarded human occupation, provided that it is left alone by the state and industry; but this hope is as certain to be disappointed as that of the more thorough-going admirers of the past. Science is too useful, indeed, essential, to the day-to-day running of modern industry to be allowed to sink into safe obscurity. Science can only live when it is the forefront of human activity. What is needed is a more thorough analysis of those characters in scientific work that make for initiative in discovery and theory, and for critical thoroughness in the establishment of facts. It has already been found in practice that it is possible to retain these characters, combined with quite expensive organization, as long as the scientists are given responsibility and allowed to arrange their own work. What has been done and is being done for war could be done equally well for peace. The world of science has fortunately always been free from many of the mercenary motives that hinder cooperation in other spheres of life. Democratic collaboration is the essence of the work of a laboratory or the study of a whole range of natural phenomena.

One implication is that science can no longer stop short at establishing facts. It must go on to see that its discoveries are adequately and rationally utilized. It was in the Soviet Union that this was first realized. There, science has for many years taken a leading and recognized part in planning the utilization of national resources to the best advantage. The assessment of human needs has led to the rational study of the best ways of meeting them and given a broad direction to the progress of scientific research. Many scientists of the old school have feared that this would lead to the destruction of pure science in favor of applied. This has not proved to be the case. Pure science is probably being studied as intensely and over as wide a field in the Soviet Union as in any other country in the world, and certainly more so than in wartime England. In war, indeed, all countries are obliged to push forward with pure and applied science together, and the very critics of planned applied science are often in the forefront of this effort.

In practice, the intellectual and material concerns of the most active leading group in the community dominate the form and content of scientific thought of the time. The seventeenth century was the age of mercantile adventure, and sciences connected with navigation and gunnery held first place. At the end of the eighteenth century the rising manufacturers directed science towards chemistry and the study of heat. In the nineteenth century, the lead passed over to electricity. In every case, science served the interests of the limited group, and its benefits to the rest of the community were incidental. The essential difference between the present and the past is that we now have the possibility, and indeed the necessity, of organizing consciously what had before merely occurred by the interplay of social forces.

To organize consciously the machinery of civilization puts a much greater responsibility on human beings than they have had in the past. As long as no one is capable of tracing out the general effects of human actions the most terrible consequences can occur, and no one will be to blame. Indeed, the classical economists had always been able to demonstrate that crises were quite accidental by-products of a fundamentally sound economic system. But once man consciously takes charge of the general organization of production and distribution, the governing powers can rightly be held responsible for any failure. But we are still far from an ordered economic system, planned for the general good, and a long struggle lies between us and its achievement. Nor can the benefits that an ordered society will bring be achieved all at once. The task is one enormously greater than any man has before attempted. That any solution is possible is due only to the development of scientific technique and scientific methods. The technical possibility of human organization on a world scale is already there. We know how to make the goods, how to distribute them, and how to ensure the necessary communications. Even more valuable is the knowledge science has brought as to how to study and measure such a vast and complex thing as the changing needs of a human society.

The consciousness of the unity of mankind as an effective working community can only be realized by the use of science. The danger inherent in the present situation is lest an attempt should be made merely to utilize science in a limited way to serve special ends. But science used in this way can only result in increasing the misery and difficulties through which civilization is passing. Knowledge is not something to be harnessed like inanimate power. If it is, the stupidity of the end outweighs the technical excellence of the means. To reap the full benefits of science, there must and can be an intimate relation between science and social processes at every stage; in assessing needs, in studying and modifying social forms, in production and distribution problems, and finally in keeping guard over the results of its application to see that they do not turn in unforeseen and undesirable directions.

For that, the scientist must be in close, free, and friendly relation with the democratically ordered state machinery, and the people at large must have an adequate understanding of the possibilities and limitations of science. At present, science is far too much regarded as a mysterious production of magical results. The object of any attempted popularization of science should no longer be, as it rightly was in the nineteenth century, to acquaint the public with the mere facts of scientific discoveries. Far more important now is to relate those discoveries to their applications in ordinary life. This is a matter for education and publicity. Science has never taken the place it should in our educational scheme. It needs to be worked in at every stage and related throughout to the interests of each age of student.

Any realistic picture must, however, point out not only the possibilities of science, but the factors preventing those possibilities from being realized. There is today a growing concern with a better world after the war. But to consider the shape of such a better world without considering the obstacles that lie in the path of its realization is simply escapism. We can study those obstacles best, not projected in an imaginary form, but here and now.

Private and institutional greed, the desire to preserve orders and ranks in a society that has outgrown them, have been potent factors in the past, and are potent factors still, in delaying progress. Unless they are dealt with, and dealt with now, there is no chance for any better world.

That is the major practical problem of our time, and it is a social and political one. It will be solved by the people themselves. But the technical forms of the solution and the rapidity with which it will be possible to achieve a better world will demand science; and, for that reason alone, the people need to know and to understand, possibly better than the scientists themselves, what modern science is and how it works.

FREEDOM OF RESEARCH

By F. W. PRESTON

PRESTON LABORATORIES, BUTLER, PA.

THE practical applications of science have reached such a development that they may fairly be said to be the dominating feature of our civilization. Progressive industrial firms are engaged in research in a big way. A large number of technological societies has grown up, whose immediate purpose is to promote the welfare and progress of particular industries by encouraging research in appropriate fields and providing media for publication and discussion.

To be technological does not involve being unscientific. In fact, in *method* one has to be scientific. The distinction between science and technology is one of *purpose*. Technological research is consciously directed to utilitarian ends: it seeks a profit. The profit may be financial profit of an industrial firm, or more broadly the enrichment of society. The "profit" may even take a negative form, so that the motive becomes not the enrichment of our own national society but the impoverishment of an enemy society, which is the basis of much technological research today. The profit, whether positive or negative, resides in the application or physical embodiment of the results of technological research, for which reason it used to be called "applied" science, a term that seems destined to be superseded by the term "technology," which is Greek for the science of the arts and crafts.

There is a growing, and dangerous, school, which comprises most of the people who know no science and some who know at least a little of it, which argues that science originated in social needs, is justified only by its works, and that its works have to be utilitarian in character to be justified. Thus unless there is some clear national benefit to be derived from a research, it is to be regarded as unsocial, unwarrantable, and in fact intolerable. This view is immature, uninformed, and pagan; it is the view of the collectivists, fascists, communists, and politicians.

There is a maturer school, less vocal, which considers scientific research to be a totally different thing. This I might call the Liberal school, as opposite to the Collectivist. According to this older school of thought, the study of science is an education, scientific research is a mental discipline, and an understanding of Nature is the first duty of man as a rational being.

In this latter aspect, the discipline of science is one of the humanities: it is the study of an ancient language, a language older than Latin or Greek or that still older language that Adam spoke: it is the language of Creation itself, the language of the Creator.

This therefore is the principal reason for studying science: it is the most direct approach to truth. And since, to quote Keats, "Beauty is truth, truth beauty," the study of science is also the most direct approach to other sorts of perfection. In this sense science is a spiritual thing, and need not be, and cannot be, justified by utilitarian arguments. It can in fact be flatly denied that it originates in material human needs and is justified only by its social services. The reason some of us study science rather than Greek is because we are thereby in the presence of greater beauty and closer to truth, not because we can make a better living out of it.

It follows that the pursuit of science is a personal and individual matter, a matter of the conscience, closely akin to a personal choice of religion, and no more to be invaded by the State, or by society, than the right of freedom of worship, which indeed it not merely resembles, but with some men of science, actually is.

But there is a curious and special feature about science, considered as a religion, which sets it apart from Buddhism or Mohammedanism. It is not merely emotionally satisfying or even intellectually stimulating; this language of Creation is so near to the truth that Creation talks back, and offers all sorts of gifts of a natural as well as of a spiritual character. The greedy worldlings

have seen these gifts and shared in them and their appetites are whetted. It is the only part of the process they *can* see: it is the only part in which they are interested, or *can* be interested. Science can be put to practical use: there will be subsidies, and men will be allowed to work only at those scientific problems assigned to them by politicians. It is like trying to buy a religion.

The practical results that flow from science are a by-product. No other known activity produces a comparable by-product. Such riches in a by-product is certainly astonishing, but a by-product it is, nonetheless. The purpose of scientific research, of itself, is a mental or spiritual product: a mental uplift, an emotional satisfaction, an artistic creation. In this it is analogous to a great musical composition or a great painting. It needs the same frame of mind, the same exceptional ability, the same deep insight, and its primary influence on others is the same. What is produced is a message: a few cents worth of pigments on a canvas, a few black marks on a musical score, a scientific paper. To most people it means nothing; to a few, perhaps, it means a great deal.

There is a difference, however. The painting is a painting. You can sell it perhaps for a high figure—or, more likely, someone a few hundred years later will sell it for a high figure. You can hang it on a wall, and make copies of it. And that is about all you can do with it.

The musical composition can be copyrighted. You can sell it. You can play it. You can make recordings of it on a phonograph disc or on a magnetized wire. And that is, again, about all.

The scientific paper you cannot sell: you may be successful in giving it away. If it can be made into a book, you may be able to sell it. You cannot hang it on a wall in an art gallery; you cannot play it, or recite it, over the radio. Generally speaking, you cannot in any direct way exhibit it to the general public. It is most unlikely that the newspapers, the movies, the radio, or any medium effectively reaching the general public will even mention it; and if they do, they are apt to put out a garbled account of it. But that doesn't matter, because the thing is *alive*, and has a fair chance of continuing alive. It is therefore subject to the laws of living things: it will change, it will grow, it will proliferate and multiply; and humanity will reap the increase.

The consciousness that there is this long-range social implication is widespread among men of science. They are aware that they are sowing, and that others, not men of science, will reap. They do not quite know what the crop will be like, whether it will be timber or grain, for the seeds are often strange, but we know they are not tares. This confidence that the seed, though strange, is good, as God found the Creation good in the Beginning, is the basis of the scientific concept of "progress." According to the men of science, the more knowledge the better; for knowledge is power, power over evil and ill-health, power over the forces of nature; and the greatest of all evils is ignorance.

But though the deep-seated belief is there, they cannot *prove* that bread cast upon the waters will return after many days. They can point out that it often has done so in the past, and will presumably do so again. But the course it follows and the currents that carry it are beyond their knowledge and may change every day. Nor can they prove that a totalitarian will not poison the bread before it returns. They only know that normally he fails to do so. But since they cannot prove these things, and men of science like to talk only about what they know and not about what they believe, they are somewhat unanimously mute on the subject, and give no reason for the faith that is in them.

This has resulted in the charge levied against them, that in the present era the men of science are the only ones that have anything to say, and the only ones that don't say anything.

An unfortunate situation then results. Since the talkers are the only ones that get listened to and those that talk from balconies and other high places are regarded by the mob below as especially oracular, false prophets are the order of the day. And one of their favorite heresies is that science is a fairy godfather, not a stern and exacting tutor; a dissipation, not a discipline; a cart-

horse to be harnessed wearily to the dull, dumb weight of a fat and indolent society, not a Pegasus coursing across the skies to do battle with the powers of darkness.

I do not pretend that every young chemist, physicist, and biologist is animated by wholly idealistic considerations, but it is certain that some of our most outstanding ones were. The greatest experimenter of them all, Michael Faraday, sought such a career because he thought it noble and idealistic, whereas the life of commerce, to which he was apprenticed, seemed sordid and trivial. To put it mildly, it would be unwise to underestimate this streak of idealism; science is the best outlet that adventurous and idealistic youth has in the present world. It would be foolish in the extreme to assume that these men are dull clods to be regimented by totalitarian-minded jurists and politicians.

From the point of view of the men of science, neither governments nor industries have shown too great an interest in science, or in men of science, except as something that can be exploited. Both governments and industries are far too largely dominated by men whose chief skill is in a battle of wits with other individuals, not in testing their brainpower against the problems of nature; their experience, as a rule, has been largely, even mainly, with strictly self-seeking individuals and groups. Perhaps this is necessarily so, and must continue to be so, for they have to deal with the whole public, and not much of the public is either altruistic or intellectual. Men of this lawyer type of mind therefore serve useful purposes in both places, but they are, with rare exceptions, automatically unfitted to deal with men of science, whose motives, so far as they are men of science, are quite different. It was said of Napoleon that he could see through any man, except an honest one, and most politicians and many "practical" men are in the same predicament, though they are not so smart as Napoleon.

But this shortcoming works both ways. If the lawyer cannot understand the man of science, neither can the scientist understand the lawyer, or that element in the population which is the lawyer's special province. Neither, as a rule, can the man of science understand the dull-witted part of the population. He may dismiss them as stupid, but more likely he dismisses them as just uninteresting: and even if they are of average intelligence, he may still find them boring. This has led to an aloofness on the part of scientists, which doesn't do them any good. The man in the street may feel that the lawyer and politician are not above exploiting him on occasion, but he tolerates them because they are good mixers. He nowhere feels that men of science are his exploiters, but he can hardly tolerate them at all, because they are superior, snobbish, and aloof. In a contest between lawyers and scientists, the man in the street will vote for the lawyers, because though they may bear careful watching, they are more comprehensible. We'd rather bear those ills we have than fly to others that we know not of. And this judgment of the average man is often right.

Now the pursuit of science for its own sake may be a noble career, but it will not of itself keep the wolf from the door. Therefore it has usually to be combined with other activities. Until very recently, I think it would be fair to say that half of England's outstanding men of science were amateurs; noblemen or lesser country gentry, who devoted themselves to science as a hobby—such men as Darwin and Lyell, for example; army officers, naval officers, and merchant venturers; and not least, parsons and clergymen. Others were professionals, in the sense that they were university professors, though some might be professors of Greek doing scientific work in their spare time; and some were true professionals, teaching and doing research in the same subject, or employed in the great museums and institutions. In our own country a not dissimilar state of affairs existed, though on a smaller scale. There were some very able amateurs, country gentlemen like Jefferson, tradesmen like Benjamin Franklin.

But today, on both sides of the Atlantic, science has been largely professionalized. There are many more people devoting their whole working day to it than could be the case unless they were paid for so doing. This of itself is not to be regretted. It is no worse than paying Congressmen or Members of Parliament for their time, a thing that was

not done till recently. It opens the field to the poor, though at the same time it lowers the average caliber of the members, or at least may tend in that direction.

The greatly increased number of research workers, remunerated research workers, comes about through the discovery by industry more particularly, and by the general public and government to a less extent, that "research pays." It pays, in their eyes, not in improved culture, in a wiser world, but in a more material fashion. It produces electric lights, telephones, and motion pictures; radio, radar, and television; automobiles, airplanes, and helicopters; X-rays, sulfa drugs, penicillin; fewer discomforts, less ill health, longer lives and more entertaining ones. The people are prepared to pay for these things, and industry is prepared to buy research as one of its raw materials, on the same basis as it buys coal, oil, or ore. It doesn't buy them as a speculation, but with the definite intent to turn them into something else and sell the product at a profit.

This sort of research is technology. As a rule, a new method, however profound and intellectually stimulating which research might disclose, of determining the distance of the moon, would not be technology, because it has no commercial possibilities. From the point of view of the Collectivists, such a research, even if successful, is a foolish waste of time, and ought to be discouraged, or even forbidden: a man clever enough to do such research ought to be doing something more useful, something to improve the lot of the poor, or to land projectiles with greater precision on enemy cities.

A research, such as is now active in many observatories, to plumb the farthest depths of the universe and try to draw a meaning from the extragalactic nebulae; an expedition to verify the prognostications of the Theory of Relativity, or, indeed, the whole Theory of Relativity itself; these are folly, and akin to crime, according to this school of thought. A scientist should have his field of investigation assigned to him by the politician: if he goes outside that field, he should be "liquidated."

Now it *is* true there is a desperate amount of short-range research that needs to be done: it *is* true that the poor are too poor, that the richest of nations are too poor. It *is* true that health is not what it should be, that nutrition is a neglected science. It *is* true that all sorts of doodads and marvels from crooning radio singers upwards can be brought into our home by television and electronics. But it is also true that a man who wants to work on the extragalactic nebulae had better be allowed to do so. He may not do any good at nutrition or be very enthusiastic about crooners. And if he isn't enthusiastic, he won't get far.

What is more, we have no means of knowing that study of the remoter stars may not, in the long run, provide the key to most important industrial and social developments. It may provide the key to our sources of power when coal has run short or is needed for more subtle purposes than burning in a furnace. There is no means of telling which researches will prove, in the end, to be the most important. A chance observation by an alert and intelligent observer following his own thoughts, and not having his thoughts prescribed for him by a politician, is apt to be more important than any set program. That is how penicillin came into the world.

Therefore there is needed a maximum of freedom to investigators, and no outside authority to say that some researches are a waste of effort. Some researches will be a waste: some scientists will exercise poor judgment. But they will, on the average, not exercise such bad judgment as outsiders.

Now the moral of all this is that the great numbers of technologists who are doing research must not be directed too closely. They must turn out enough "pot-boilers" to earn their keep, but they must be allowed to create permanent works of art also, if they can. I do not advocate coddling them, or making life too easy. Masterpieces have been produced in a garret and worse places. But any industrialist or any politician who should undertake to say in advance that a research worker must apply himself solely to a given line of thought, or directed solely to a particular end, would be both presumptuous in the extreme and an enemy of society. For the great discovery will not come to those who say "Lo, here," or "Lo, there," but as a thief in the night.

Years ago, when I first began to do re-

search work in a very amateurish fashion, I had the good fortune to be associated with another young man, a brilliant mathematician. We were working on separate problems, for the same company, and we both made the same discovery. Most often the things we worked at didn't work out: the by-products did. We decided to spend about half our time on the things the firm thought it wanted and half on anything else which was suggested to us, as a promising line of investigation, by these primary researches. I suspect we have been working on some such basis ever since, and that the lines we picked up ourselves have been socially and industrially the more important.

This discovery was not something peculiar to ourselves, nor were we the only people who ever made it. Many others have made it independently. Some of the outstanding industrial research laboratories have discovered and endorsed it. When you have a first-rate man who sees a problem that attracts him, don't try to switch him to something that looks more important to you. Leave him alone, encourage him, and leave him alone some more. "Something is bound to happen," as Dr. Whitney once told me. This policy has proved itself many times, but only with first-rate men. Not all the men who drift through our scientific schools and finish perhaps with a Ph.D. can be regarded as first-rate. They are not all Langmuirs and Steinmetzes.

Anyone who is doing really original research is of necessity a man of very high I.Q. There is, in fact, no other occupation that calls for so high a rating. It is intellectually the most gruelling course there is. It is pioneering: it is exploring. And those who wander off into the trackless wilderness, not knowing whether their obstacles will be mountains or swamps, hurricanes or doldrums, cannibals or microbes, need to be a particularly tough and resourceful lot.

In institutional or group research, as in any other group activity, a fundamental human craving comes into play. The leader must be the same sort of man as his followers. Brave men cannot be led by a coward; highly intelligent men cannot be led by one of only average talents; research workers cannot be led for long by men who have not themselves won their spurs in research. The leader of a band of soldiers must himself be a soldier; a cavalry charge cannot be led by a man who cannot keep his seat; airmen need an aviator at their head; and the principle is very generally true. A banker or promoter may be able to get money for a research organization, but if he does not understand research, including the technical aspects of it, he cannot inspire his men with confidence, still less with enthusiasm.

Robin Hood is said to have made it a rule that he would accept no followers who were not better men than himself, and this careful culling of applicants for a life in Sherwood was no doubt wise. He would otherwise have had a lot of ne'er-do-wells and weaklings on his hands. But it is a safe bet that Robin's rigid specifications and tests prove him to have been himself a very exceptional leader, and capable of directing the best men in the greenwood.

A leader is much more important than money, important though money is. How much money would have replaced Churchill in the Battle of Britain?

I have stressed some of these factors because there is still a tendency to assume that research is something that can be bought, like a drill-press or a drill-press operator. Industrialists frequently fail to understand the point, and so far as politicians are concerned the error runs clear through the first Kilgore Bill. When you can call a Churchill into existence by voting so many million dollars, then you can call true scientific research into existence by the same route: but not before.

We come back then to the same central problem: the public, the industrialist, and the politician appreciate technology but do not appreciate science as such. Probably they never will; but if they can be induced to keep their hands off, and not wring the neck of the goose that lays the golden egg, the golden eggs will continue to be forthcoming. Penning the goose up and stuffing it may produce *pâté de foie gras*, but it will not increase the egg supply.

A more favorable attitude towards freedom in science, including technological science, on the part of both industry and government is the best guarantee of progress. Industry is "sold" on chemistry, and to a

less extent on physics. The electronics of this war have pretty well "sold" it on physics by this time, and physicists are in short supply. But there is still an almost complete lack of appreciation of biology, except where it deals with microbes, and even that field is left largely to the medical, rather than the industrial, world to worry about.

I am of the opinion that any laboratories set up by individual firms, or by groups of firms, should have the broadest possible terms of reference. The country will benefit. The possibility that a research organization supported by the glass industry may discover something useful to the refractories industry, the mining industry, or even to people who are not industrialists at all, is not a calamity. Even if the plaudits should come from so remote a group as the astronomers, no disaster has occurred. The bread has floated off a long way, but it may still come back—buttered.

By giving a maximum of freedom to investigators, by encouraging them to take the widest—and perhaps the wildest—views, the greatest benefits will accrue. By going off in exactly the wrong direction, as nontechnical minds see it, the greatest discoveries are made. That is how Columbus discovered America.

THE BOWMAN COMMITTEE SPEAKING

The committee foresees that an increased measure of Federal support will raise new problems. We have, therefore, carefully considered the possibility of increasing Federal aid for scientific research without, at the same time, introducing undesirable paternalism. For, in order to be fruitful, scientific research must be free—free from the influence of pressure groups, free from the necessity of producing immediate practical results, free from dictation by any central board.

Many have been impressed by the way in which certain fields of applied science have benefited, during the war, from an increased measure of planned coordination and direction. It has thus been very natural to suppose that peacetime research would benefit equally from the application of similar methods. There are, of course, types of scientific inquiry that require planning and coordination, and a large degree of control is inevitable and proper in applied research. However, there are several reasons why pure science in peacetime cannot wisely or usefully adopt some of the procedures that have worked so well during the war. War is an enterprise that lends itself almost ideally to planning and regimentation, because immediate ends are more rigidly prescribed than is possible in other human activities. Much of the success of science during the war is an unhealthy success, won by forcing applications of science to the disruption or complete displacement of that basic activity in pure science which is essential to continuing applications. Finally, and perhaps most important of all, scientists willingly suffer during war a degree of direction and control which they would find intolerable and stultifying in times of peace.

It is the belief of this committee that increased support of research in American universities and nonprofit institutes will provide the most positive aid to science and technology. But we do not believe that any program is better than no program—that an ill-devised distribution of Federal funds will aid the growth of science. Our concrete proposals seek to augment the *quality* as well as the quantity of scientific research. We believe that there are historical precedents of Government aid to research, both in this country and abroad, which show the possibility of providing, within the framework of sound administrative practice, sustained nonpolitical grants which would operate in such a manner as to call forth from existing institutions even greater initiative, effort, and accomplishment.

The organization or instrument finally set up should not attempt to play the role of an all-seeing, all-powerful planning board trying to guide in detail the normal growth-processes of science. The first and most essential requirement is that the groups administering a program of research assistance be composed of men of the highest integrity, ability, and experience, with a thorough understanding of the problems of science. The committee believes that an independent Government body, created by the Congress, free from hampering restrictions, staffed with the ablest personnel obtainable, and empowered to give sustained and far-sighted assistance to science with assurance of continuing support, would constitute the best possible solution.

It is our belief that the desired purpose can best be served and the possible dangers minimized by centering the responsibility for this program in a new organization, a National Research Foundation, whose function should be the promotion of scientific research and of the applications of research to enhance the security and welfare of the Nation.—From *Science, The Endless Frontier. A Report to the President* by VANNEVAR BUSH. Washington, July 1945. pp. 73, 74.

SCIENCE ON THE MARCH

THE GENES OF PLENTY

EARLY in the present century the shadow of black rust lay over the wheat fields of South Dakota. The disastrous rust epiphytotic of 1904 left a profound impression on the farm lad Edgar McFadden in this, his first year of farming. He resolved to find some means to avert the rust scourge, and to that end he sacrificed to acquire such knowledge of farm science as was offered by the state agricultural college and further extended this by assisting in the work of the South Dakota Agricultural Experiment Station. Convinced that the only prospect of gaining real relief from rust damage lay in the development of rust-resistance in the wheat plant itself, he cast about for some means of incorporating such resistance into the high quality but rust-susceptible northern wheats.

Yaroslav emmer, a feed wheat quite worthless for bread but highly resistant to the two destructive species of wheat rust, seemed to offer some promise as a source of rust-resistance, and in 1916, another of the years of devastating rust, McFadden, then an undergraduate student, delicately removed the unopened anthers of an emmer blossom and dusted the stigma of the blossom with pollen from a flower of the high-quality but rust-susceptible Marquis wheat.

Emmer has 28 somatic chromosomes and common wheat 42, so, as would be expected, such a hybridization resulted mainly in sterility or poorly developed seed. When these were planted, only a single seed developed into a wheat plant, but it was a precious plant, the descendants of which are growing today on millions of acres.

For the next nine years McFadden, mainly on the mortgaged South Dakota farm where he was striving to earn a living under the adversities of drought, hail, and rust, carefully tended the growing progeny of this single seed. There were times when the farm was neglected or when the rust which he artificially liberated to test the resistance of his hybrids escaped to the fields upon which his living depended. But by 1925 the best selection of this hybrid progeny, H49–24, had shown a consistent record of high rust-resistance, and this record was maintained in subsequent tests at 50 experiment stations in the United States and Canada. McFadden named his new wheat "Hope."

Combined with some desirable qualities, Hope had certain serious defects such as low yields, low test weight, and frost susceptibility, so that it has never become of commercial importance. But as a breeding parent, compatible with common wheats and passing its rust-resistance to hybrids with the latter, Hope has an unparalleled record in contributing to worldwide wheat production. Many of the newer American wheat varieties, such as Austin, Mercury, Merit, Mida, Newthatch, Pilot, Regent, Renown, Rival, and Vesta contain the blood of Hope or its sister selection, H–44.

The enormous contribution that Hope, through its offspring, has made to the American food supply and farm income is brought out in figures recently released by the Canadian Department of Agriculture. It was shown that in two Canadian provinces alone the development and introduction of rust-resistant wheats, principally Hope descendants, has increased the *annual* value of the wheat crop by $27,242,000, an amount more than thirteen times greater than the *total* expenditure ever made by Canada on rust research.

Wheat is but one of many crops that have been saved or made more productive through the recent development of disease-resistant varieties. The sugar beet is another. With the stimulation of the founding of the beet sugar industry in France under the aegis of Napoleon, pioneering attempts were made to establish a similar industry in America, and while some of these failed, notably that of the Mormons in Utah, successful beet sugar production began in 1870, and slowly increased.

But beginning in 1898 this promising young industry began to feel the throttling influence of a new disease of beets, curly top. The growing devastation of the curly top virus soon gravely threatened the future of beet sugar production in the western states.

Factories valued at millions were closed and began to fall into ruin as abandonment of sugar beets, a crop sorely needed for diversified farming, rapidly increased. Vast areas suitable for beet culture remained undeveloped. In the Twin Falls area of Idaho some 21,000 acres were planted with sugar beets in 1934 and nearly 19,000 of these were abandoned, largely because of curly top, while the yield from the small acreage harvested was only one third that of a healthy crop.

The beet sugar industry owes its rescue from impending extinction largely to the successful breeding and selection work of plant pathologist Eubanks Carsner and his associates in the U. S. Department of Agriculture. After many years of research the first variety of beets resistant to curly top, "U. S. No. 1," was made available to farmers and planted on 35,000 acres in 1934. This was only moderately resistant but it revitalized the beet industry of the Intermountain area. It was soon followed by progressively more resistant varieties, culminating recently in "Improved U. S. 22." The acid test came in the curly-top year 1940 when the Improved U. S. 22 yielded 16.6 tons per acre while the old types of beets were a total failure, and in a year of light curly-top, 1941, Improved U. S. 22 yielded 29.39 tons as compared with 5.16 tons for the old type beets. Today practically the entire American sugar beet acreage, which otherwise could hardly exist, is planted with the new disease-resistant varieties.

With sugar cane a similar story may be told. This crop, too, is plagued with a virus disease, mosaic, that causes losses sometimes exceeding 50 percent of the crop. So important is disease in sugar cane that disease reaction stands second only to sugar content in determining the value of a cane variety.

In 1914, before its cause was known, the mosaic disease was introduced into American cane plantings. In Louisiana, where conditions were favorable for the spread of the virus, production dropped steadily from 200,000 tons a year to 47,000 in 1926, a loss to the sugar and syrup industries of the South, due to mosaic, of $100,000,000.

But again plant pathologists and breeders were awake to the situation and even before this all-time low had been reached, they were busy at the long, slow task of incorporating mosaic resistance into high-yielding cane varieties. A world-wide search was made for the genes of resistance to mosaic. Intensive hybridization, testing, and selection have resulted in new, mosaic-resistant cane varieties that have not only re-established the Louisiana sugar industry but have even made it possible to produce more sugar at a lower cost than was possible before the mosaic disease appeared.

Disease-resistant varieties that have saved or increased production of numerous other crops are milestones along the march of science: varieties of watermelons, tomatoes, cotton, oats, beans, cucumbers, flax, indeed of nearly every commercial crop. And now, not content with developing high quality varieties that are resistant to some one major disease, the resistance-breeders are forging ahead to new achievements. Among the latest are varieties of beans that are at once resistant or tolerant to five major diseases, and varieties of cabbage that combine good adaptation, high productivity, and resistance to the deadly yellows disease, with an unusually rich content of ascorbic acid (vitamin C).

The development of these new varieties is far from being a haphazard process; it is as carefully planned and blueprinted in advance as is the construction of a complex building by a skilled architect. The new variety, to be acceptable, must have many desirable qualities. It may be necessary to search the world to find the genes bearing these qualities. When found, these may reside in varieties that contain a predominance of undesirable genes that must be eliminated from the finished product. The crossing itself may be a difficult process, as when two potential parents are sexually incompatible, requiring thousands of attempts to produce a single fertile seed, bombardment of the nucleoplasm of one of the parents with X-rays to render it more tractable, or subjecting it to the action of colchicine. Once a fertile hybrid is obtained, years of culture are required before the new variety will breed true, and these must be years in which it is subjected to all the tortures to which it may be exposed in nature. In this acid testing thousands of the hybrid progeny fall by

the way, and only a keen eye and persistent spirit can recover from the population of inadequates the rare individual which contains the desirable genes and lacks the undesirable ones of parents and grandparents.

The plant pathologist-breeder is at once the architect and the builder of the crops of tomorrow. His record is one of unparalleled achievement in the rescue and rejuvenescence of waning agricultural ventures, and his work points to far greater accomplishment in the forward march of agricultural science.—K. STARR CHESTER, Oklahoma Agr. and Mech. College.

CHARACTERISTICS OF LOCOMOTION OF WHITE BLOOD CELLS

THE motility of the white blood cells is manifest only under special circumstances. Normally, they are carried passively through the vessels by the circulating blood, in which they possess little or no active motion of their own. However, in the tissues of the organism, they are actively motile. For example, under the influence of a local stimulus, such as an infection, they leave the blood vessels and migrate into the tissues where they disperse themselves by active locomotion throughout the area of inflammation. Here they play an important role in the elimination of the injurious agent. The locomotory properties of the white blood cells may also be demonstrated by experiments in vitro. This was first done in 1846 by the English anatomist T. Wharton Jones. Since then numerous investigators have confirmed and also extended Wharton Jones' original observations.

There have been many attempts to use the locomotory behavior in vitro as an aid in identifying certain types of white blood cells, particularly the immature forms at the sites of blood formation. The white blood cells are divided into two main groups: the granulocytes, whose cytoplasm contains numerous small granules, and the agranulocytes, which have no granules. The granulocytes are subdivided into three groups: heterophile leucocytes (65–75 percent), eosinophile leucocytes (2–5 percent), and basophile leucocytes (½–1 percent). These three groups are distinctly separated on the basis of their nuclear form and the morphology and staining reactions of the granules. The agranulocytes include the lymphocytes (20–25 percent) and the monocytes (3–8 percent). In contrast to the granular cell forms, the differences between the latter two groups are primarily quantitative. Many cells in the blood are of an intermediate character and cannot be classified with certainty as either lymphocytes or monocytes. The matter of classification becomes even more difficult when one considers the precursors of the white blood cells. The granulocytes are formed in the bone marrow from nongranular stem cells, which closely resemble the larger lymphocytes. In fact, absolute criteria for distinguishing them in all instances from the lymphocytes are lacking. Thus, the question arises whether the stem cells in the bone marrow are identical with the lymphocytes or whether they are to be classified as separate cell forms. As mentioned above, attempts have been made to clarify these problems by studying the locomotory properties of the various blood cell types in vitro. Such work has been done on drops or thin films of blood, with or without the help of supravital staining methods. However, according to the present consensus, the motility of the cell is not a reliable diagnostic criterion in such preparations. For example, such investigations have failed to produce unanimity of opinion regarding the motility of the lymphocyte. Some workers believe this cell to be entirely nonmotile, whereas others feel that the lymphocyte can produce pseudopodia and change form but cannot migrate progressively. Still others are of the opinion that this cell can migrate very slowly. Apparently many uncontrollable factors influence the motion of the white blood cells in simple drops or in thin films of blood. In contrast to this, observations on the motility of white blood cells in tissue cultures, with which we will deal here, are more consistent. In tissue cultures the cells are suspended in a clot of blood plasma and tissue extract. This medium provides the nutrient substances and also serves as a solid substratum for the cells to move on. In such cultures it has been definitely established that all white blood cells are capable of active motion. Furthermore, it has been found that in tissue cultures different types

of white blood cells exhibit constant and characteristic types of motion. Another important technical advance was the introduction of time-lapse motion photomicrography by means of which an accurate analysis is possible of phenomena which are not accessible for study by direct observation.

The characteristics of motion of the various blood cell types can conveniently be expressed in terms of polarization of activity of the cytoplasm, as will be seen from the following descriptions. It is emphasized, for reasons to be discussed later, that these descriptions refer to the motion of the white blood cells on a flat surface.

The migration of the *lymphocytes* in tissue cultures is characteristically intermittent. Periods of locomotion alternate with nonlocomotory phases. During the locomotory phase, the lymphocyte has the general shape of a hand mirror. There is a round cell body, which contains the nucleus, and an active anterior part where the pseudopodia are formed. A small amount of cytoplasm drags behind like a tail. This indicates that the cytoplasm of a lymphocyte in motion is differentiated into an active anterior part and a passive posterior part. In other words, the cell is polarized. The strict polarization of lymphocytes manifests itself also in their pathway of migration, which has the appearance of a wavy line with smooth curves. Abrupt changes of direction rarely occur. During this locomotory phase, lymphocytes have been found to move at a rate of 33.09 ± 0.78 micra per minute. Such high rates of locomotion have previously been thought to occur only among heterophile leucocytes. In the nonlocomotory phase the cell changes its form. The anterior pseudopodial area and the "tail" are withdrawn, and the cell, which was previously shaped like a hand mirror, assumes a rounded contour. However, this nonlocomotory phase is not merely a "period of rest." Small pseudopodia continually arise from this rounded cell body; the pseudopodia are extruded, not from a limited area, as in the locomotory phase, but from all sides of the cell body. Since in this state the cytoplasm of the lymphocyte is not differentiated into an active anterior part and a passive posterior part, this phase has been termed "depolarized." No progressive locomotion takes place during this depolarized phase, but the cell moves irregularly about over short distances which rarely exceed the width of the cell.

The direction of the motion of the lymphocyte in the locomotion phase seems not to be related to an external stimulus. In any given area of the culture, the directions of the movements of the cells are completely haphazard, frequently crossing one another's path or moving in exactly the opposite direction. After a depolarized phase, migration may proceed in any direction and the new pathway of migration may form a sharp angle with the previous one.

The *heterophile leucocyte* in motion has essentially the same shape as the lymphocyte, but characteristically is not as strictly polarized as the latter. Although the cytoplasm of these cells in motion is differentiated into an active anterior and a passive posterior part, these areas are not as distinct from one another as in the lymphocytes. The pseudopodia are not limited exclusively to the anterior area of the cell, but often appear at its sides. The tail is relatively smaller and not as constantly present as in the lymphocytes, but is frequently withdrawn so that the cell body assumes a blunt end. The result is that the hand mirror form, although fundamentally present, is less pronounced than in the lymphocytes. The less rigid polarization of the cellular activity is also shown in the pathway of migration. Sudden changes of direction, and even complete reversals occur frequently so that the pathway of migration has many sharp angles.

The *macrophage* is a close relative of the blood cells. This highly phagocytic cell is present in most of the tissues of the body. it has been shown, however, that in inflammatory processes and also in tissue cultures, macrophages can develop from agranular blood cells. In contrast to the above discussed agranular cell types, the macrophage has no progressive locomotion to speak of. The cytoplasm of these cells is continually depolarized. Pseudopodia arise from all sides of the cell body and are subsequently withdrawn. During this process, the cell moves irregularly about and changes its position very slowly, covering distances of about 15 micra or less in one hour.

These types of motion may all be classified as random movements. Even the motion of the lymphocyte, which in the locomotory phase has a directed character, is a random movement, since after a depolarized phase the motion may proceed in any direction.

In order to move, the cells require a solid substratum. If they are suspended in a liquid, no locomotion is possible since white blood cells do not swim but crawl on a solid substratum. This may account for some of the divergence of opinion on the motion of leucocytes in drops or films of blood. In a tissue culture the substratum can be either the flat surface of the glass coverslip on which the culture is made or the fibrin network of the plasma clot. The above-described movements are seen only when the cells are adhering to a flat surface. Within the plasma clot their motion is entirely different. Here the lymphocytes as well as the heterophile leucocytes have an elongated cylindrical form and move in a twisting, writhing manner, which has been termed "wormlike." The nature of the fibrin substratum forces the cell to assume this elongated shape, while the movement around the fibrin threads through the spaces available in the three-dimensional network determines the wormlike character of the motion. The anterior pseudopodial area and the passive posterior part are still present, but are less conspicuous owing to the narrowing of the cell body. The complexity of the fibrin network makes it unsuitable as a substratum for studies of locomotion.

The fact that different types of blood cells have characteristic types of motion has been applied to one of the most fundamental problems in hematology: the nature of the blood stem cells in the bone marrow. According to one group of investigators, these cells, from which the granulocytes and also the red blood cells arise, are identical with the lymphocytes, which develop from pre-existing lymphocytes in the lymphatic tissue. This view, that the bone marrow cells and the lymphoid cells have a common stem cell, is the basis of the "unitarian theory" of blood formation. Other workers uphold the "dualistic theory," that the bone marrow stem cell is a specific cell type and that both cell lineages have separate stem cells called, respectively, myeloblast and lymphoblast. A great many histological and cytological techniques have been applied in order to find sufficient morphological differences to justify such a separate classification of the two stem cells. Studies on the motion of these cell types on a flat surface have shown that the locomotion of the myeloblast, as well as that of the lymphoblast, is identical with the motion of the lymphocyte. This supports the "unitarian" rather than the "dualistic" point of view.

The changes in locomotion which occur during the development of stem cells into mature blood cells have also been investigated. In the development of heterophile leucocytes from bone marrow stem cells, only relatively minor changes in locomotion occur. As indicated above, the motion of heterophile leucocytes differs from that of the lymphocytes only in its degree of polarization. Since the movement of the bone marrow stem cell is identical with that of the lymphocyte, the intermediate cell-forms merely have an intermediate degree of polarization. The red blood cells and their immediate precursors do not exhibit any active locomotion. However, the cytoplasm of very young red blood corpuscles with only a small amount of hemoglobin (polychromatophile erythroblasts) shows some activity. The nuclei of these precursor cells have a reversible rotating movement through a short arc, while the nuclei of the cells containing more hemoglobin do not show this phenomenon, but are completely quiet.

More striking than the changes in locomotion during the development of blood cells are the changes in locomotion which accompany the transformation of lymphocytes into macrophages, which occurs in tissue cultures and also in inflammation. The fundamental morphological change in this transformation is a gradual hypertrophy of the lymphocyte. Physiologically, a change in type of motion and also the acquisition of phagocytic properties are outstanding. In this transformation, intermediate cell forms occur which have the same intermittent motion as the lymphocytes. During the locomotory phase their motion is identical with that of the lymphocytes, while in the depolarized phase, it progressively assumes the characteristics

of macrophage motion. During the later stages of this transformation, the locomotory phase disappears, and the continuously depolarized motion characteristic of macrophages remains. It is interesting to note that the movement of the lymphocyte in the depolarized phase, although superficially greatly different, is fundamentally identical with the motion of the macrophage. Both movements are characterized by their depolarized nature and the lack of progressive motion, the difference between them being primarily quantitative. The macrophage possesses absolutely and relatively more cytoplasm and extrudes more and larger pseudopodia than the lymphocyte in the depolarized phase.

This work on the movement of blood cells may be considered a result of the growing trend in histology toward the study of the physiological behavior of cells by observing them in the living state. The inaccessibility to observation of the cells, in their normal environment of the tissue, has led to the application of the tissue-culture technique. There is no doubt that the conditions in tissue culture are different from those in the living tissue. One has but to point to the artificial nature of the substratum, which serves the cell as a support in the culture, to realize that all the results thus obtained cannot be assumed for cells in their natural environment. However, the fact that differences in type of motility occur in tissue cultures may suggest that such differences, although perhaps of a different nature, also exist in vivo.—P. P. H. De Bruyn, Department of Anatomy, University of Chicago.

ARMY UNIVERSITY CENTERS IN EUROPE AND THE SCIENCES

The shift in the enlistment of science from that for physical destruction to that of mental restoration was put under way by the establishment of the Army University Centers in Europe. One opened in Shrivenham, near Oxford, England, to about four thousand soldiers of prewar university caliber in late July; a second one had its formal opening in France in mid-August under the name of Biarritz American University; and a third is in action at Florence, Italy.

It is unique that these University Centers should come into full-being within a few months, and each with a faculty of over two hundred, of which about 30 percent are teaching scientific subjects to almost six hundred students, as at Biarritz, for example. When 65 percent of the faculty are drawn from civilian life in universities and colleges, when more than 50 percent have earned the doctorate, and when all are above the age of forty-two, such experience and abilities of the faculty assure successful science teaching in this new institution. This faculty—obtained by selection and not by application, or even political appointment—is offering almost one hundred different courses in the sciences. Classes are in sections of few enough students to facilitate student-teacher contacts and free exchanges of ideas.

The students, too, are selected with reference to their educational backgrounds for university work. Their attitudes are excellent, their minds open, their diligence beyond expectation, and already their shifts from wartime to peacetime thinking are evident. They have each enrolled for no more than fifteen classroom hours per week. They indicate a distinct mental maturity in consequence of their time in the Army. For them the war experience has also been an education, and they are now transferring the better values from it to this classroom venture in areas where recently the war raged. Though after eight weeks they go to parts of Europe to be replaced in each center by another group, they are after all equipping themselves to go back into competition with the society at home.

Shortages of apparatus, crowded and unfinished classrooms, insufficient reference books in the library, and other similar material handicaps at the outset, or even personal discomforts, are not stopping the march of this education for peace. Here is democracy on its way back, as captain and private match wits in their discussions of lessons in science. —Wm. A. Albrecht, Biarritz American University, France.

BOOK REVIEWS

SCIENCE THROUGH THE AGES

The Autobiography of Science. Edited by Forest Ray Moulton and Justus J. Schifferes. 666 pages. 1945. $4.00. Doubleday, Doran and Company, Inc., New York.

THE purchaser of this interesting volume will be the wiser, first, from studying it as an anthology of not only fine but highly significant literature and, second, from reading its preface carefully.

The chief problem in an anthology is that of selection. Dr. Moulton and Mr. Schifferes have done well. Their original outline contained some three hundred names with material enough to fill a dozen books. The difficulty of meeting their standards is implied. Too many conflicting conditions had to be met. It was desired to offer the most important contributions in science. At the same time it was realized that much that is scientifically significant is beyond the capacity of the general reader for whom the volume is devised.

About ten dozen authors and sources are considered in varying detail from a few lines, as in the case of D. I. Mendelyeev (1834–1907), to twenty or more pages, as with G. Galilei (1564–1642). Choice was made on the basis of readability as well as from the standpoint of scientific significance. The result is uneven, as far as scientific interest is concerned. Many important scientific contributions which would occur promptly to any historian of science are omitted.

The editors remark on the difficulties of choosing representative passages from great scientific contributors. Their dilemma is pleasantly illustrated by reference to their consideration of von Helmholtz (1821–94). Choice was influenced by their desire to refer the relationships of scientific activity to other interests of cultured people. Significant scientific contributions were thus sacrificed for more general philosophical or literary efforts expressing the scientific spirit.

While the editors realized the difficulties of making a representative selection of significant scientific contributions, they apparently failed to realize the significance of making such a selection at all, particularly with respect to scientists still living. An anthology is the first step in the establishment of a canon. Unsophisticated readers get the impression that what is included in the selection is the only material in the field that is worthwhile, and that whatever is omitted is not worth including. The result is that canonical tendencies develop and unfortunate consequences, wholly foreign to the spirit of science, appear. So many factors are operating today to establish a "scientific canon," disagreement with which is heresy, that every care is justified to prevent such an undesirable possibility.

The material in the volume is arranged chronologically. It also deals exclusively with the development of what is termed "Western science." This is an unfortunate distinction to attempt to draw. The science of antiquity is covered in thirty-three pages: from the speculations of Genesis to Galen (A.D. 130–201) and including extracts from Egyptian medical papyri, Hippocrates (460–377 B.C.), Aristotle (384–322 B.C.), Archimedes (287–212 B.C.), Lucretius (98–55 B.C.), Pliny (A.D. 23–79), and the great Roman architect Vitruvius.

The sleeping period of science includes material from Roger Bacon (1214–94), Guy de Chauliac (1300–1370), and the famous "Regimen of Salerno." The reawakening of science in the Renaissance covers some seventy pages and offers extracts from Leonardo da Vinci (1452–1519) to William Gilbert (1540–1603).

The increasing accumulation of scientific activity is shown by the devotion of nearly a hundred pages to the growth of science in the seventeenth and early eighteenth centuries, from Francis Bacon (1561–1626) to Carolus Linnaeus (1707–78). The maturity of science is illustrated by abstracts from publications during the revolutionary period of the eighteenth and early nineteenth centuries, proceeding from Joseph Black (1728–99) to Edward Jenner (1749–1823). The progress of science in the early nineteenth century includes reference to the brilliant

chemists of the period and extends from Michael Faraday (1791–1867) to Charles Lyell (1797–1875).

It takes a hundred and twenty pages to offer selections from the assertions of science in the age of evolution from Charles Darwin (1809–82) to William Thomson (1824–1907). And then it takes nearly two hundred pages to offer representative contributions from twentieth century scientists from Samuel P. Langley (1934–1906) to Donald C. Peattie (1898–). The volume concludes with a brief but uncritical bibliography in the history of science, acknowledgments, and a topic and name index.

The acceleration of scientific activity is clearly evident from this brief summary of the volume. Pertinent to the editors' hope that the volume will introduce many readers to the history of science are well-prepared but brief biographical sketches of the many scientists whose writings are quoted.

For satisfactory orientation in the development of science it might have been wise, however, to have included further reference to many of the great scientists which the editors felt called upon to omit. Here the problem of choice and discrimination enters. Might it not have been worthwhile to have used one instead of three quotations from Francis Bacon in order to include a single short quotation from, say, Marcello Malpighi (1628–94), who did so much to establish modern botany and zoology? One quotation from Gilbert White (1720–93) might have been enough, so that a single one of the short brilliant demonstrations of Euclid might have been added in an earlier section. Perhaps a single quotation from Darwin, Galton, or Florence Nightingale might have been enough, so that something might have been said of Virchow, Claude Bernard, Koch, or Ehrlich.

The volume seems weak in quotations from mathematicians and astronomers. Perhaps these were omitted because they are frequently so hard to understand. The editors say they were guided chiefly by two criteria: first, importance as "turning points" in the history of science, and, second, readability. But still it might have been wise to include at least a short note from Kepler, Brahe, Laplace, Chamberlin, the Herschels, Euler, Gauss, or even Cardan. Certainly it might have been wise to have quoted something from George Sarton on the humanizing of science. This might have been a little more scientifically appropriate than even the very splendid (and brief!) quotations from Donald Peattie's "Almanac for Moderns."

It is clear that the editors have been attracted more by the readability of scientists than by the strict intellectual significance of scientific contributions. The Egyptian medical papyri do contain genuine contributions to scientific progress, in addition to an "advertisement for a remedy." Hippocrates on "Fractures" or the "Sacred Disease" is probably more significant scientifically than the Aphorisms or the Oath. Valerius Cordus (1515–1544) is more significant, but duller, than Paracelsus (1493–1541) on the transition of alchemy to chemistry. Paracelsus was, however, a pioneer in psychiatry and industrial disease. Cushing on the pituitary is probably more important scientifically, but less entertaining, than he is on the facetious career of the surgeon. One of the passages of Maeterlinck, however lovely, or one of the quotations from Freud, however interesting, might have been left out so as to include a paragraph of Joseph Barcroft, or Gilbert Lewis, or Ramon Cajal, or I. P. Pavlov. Certainly Sir William Osler would have been happy to yield a page to Robert Boyle!

The editors say there are six ways to read the book: as a storybook, a history book, a textbook, a reference book, a source book, or as a chronicle. It seems to me to have best use as a reference book. But from this standpoint it is appallingly incomplete. It is also incomplete as a source book. It is really not a chronicle. As a textbook it is reminiscent of the literature anthologies which are used in some of the more progressive preparatory schools. It can hardly be called a history book. As a storybook, however, it is delightful. It is simply a popularization of some of the better known Western contributions to scientific literature.

Nothing can better illustrate the success of the editors than to quote their own appreciation: "Many of the passages in this anthology partake of the highest qualities of

literary style. Present-day 'third person,' 'passive voice' fashion in scientific communication notwithstanding, the science masters of the ages, speaking in the first person, unequivocally illustrate that 'the literature of power' and 'the literature of knowledge'—De Quincy's phrases—can be one. Good science makes good reading."

The hope of the editors will be thoroughly fulfilled: "that the book will introduce many readers to the history of science and orient them in it; that it will give them a first-hand, original account of the substance of great scientific thinking; that it will suggest the sweep, scope, and international brilliance of scientific development; that it will give them a basis for discriminating between true and pseudo-science, and finally, best of all, that it will provide an accurate, intimate, living knowledge of the great men and women of science, some insight into their characters, some sense of the true nature of their work." The volume is an excellent effort to offer the best in scientific literature and it is certainly to be hoped that the book will arouse wide interest and appreciation.

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INDIVIDUALITY IN THE MAKING

The Biological Basis of Individuality. Leo Loeb. 711 pp. 1944. $10.50. C. C. Thomas, Springfield, Illinois.

IN this masterly volume, the fruit of a lifetime of biological experimentation and reflection, Professor Loeb considers the nature of individuality from the standpoint of the biologist. For much of the basic exposition of his findings Professor Loeb relies upon the experimental work on transplantation of tissues carried out in his own laboratory by himself and his collaborators. Admirers of Professor Loeb will be particularly glad of this since here for the first time is performed something of a synthesis of a lifetime's labors. This is not to say that tissue transplantation is the sole experimental work relied upon or that the work of others is neglected: far from it. The range of subjects and material is very broad indeed, and there are few biologists who would be as competent to deal with such a vast range of material as is Professor Loeb. Transplantation experiments, serology, immunology, oncology, embryology, morphology, genetics, chemistry, theoretical biology, and finally psychology and sociology, are all so much very relevant grist to Professor Loeb's mill.

In a short review it is quite impossible to do justice to this remarkable work, a work which is bound to increase in usefulness and stature with the passage of time. For the scientist of today, no matter what field he may be interested in, and for the philosopher, the present volume should become indispensable reading. It is full of the most novel insights and generative of others. Its greatest usefulness will, doubtless, be in the rewarding field of experimental biology, but workers in such seemingly distant fields as the psychological and social sciences will benefit considerably from a study of this work, and I would here especially draw their attention to Part VIII which deals with "The Psychical-Social Individuality." Such workers will, at the very least, find this part stimulating, for Professor Loeb's views are strictly his own, and this is all to the good. Not altogether justly we may paraphrase Hobbes and say, had Professor Loeb read as widely in the social and mental sciences as other men have done he would have been as ignorant as they are. As it is he brings a fresh and original viewpoint to bear upon the analysis of the somewhat tergiversating problem of human individuality. As Professor Loeb very properly points out in the second chapter of this part "the problems of philosophy have been largely concerned with the meaning of individuality" (p. 648).

If there is one criticism I would make of this last part of the work it is that Professor Loeb does not sufficiently jettison the old mind-body dichotomy. Though he obviously does not subscribe to that way of thinking he still writes of the body *and* the mind. It would do us all a great deal of good if we could give these two confusing terms a long vacation.

I should like to conclude this very inadequate notice of a most important book with the paragraph which concludes Professor Loeb's final chapter.

"Thus, in matters which relate to man as a

psychical-social organism, it is the environment which has become a preponderating influence and which largely determines his fate. To adapt the psychical-social environment to the needs of man, so that he can function in the most adequate manner, is, therefore, the most important task which humanity has now to face."

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HERALDING AN AGE OF AGE

New Goals for Old Age. Edited by George Lawton. 210 pp. 1943. $2.75. Columbia University Press.

You Are Younger Than You Think. Martin Gumpert. 244 pp. $2.75. 1944. Duell, Sloan and Pearce, Inc., New York.

Ageing and Degenerative Diseases, Vol. XI, Biological Symposia. Various Authors. 250 pp. Illus. 1945. $3.00. The Jaques Cattell Press, Lancaster, Pa.

TIME waits for no man. We have but two alternatives: To grow old or to die young. More and more of us are being given the opportunity to choose the former course. Science has enhanced human longevity to the point where even those not yet old themselves are becoming aware of the significance of senescence and of the immensely complex and urgent problems of aging. Ultimate solution of these problems is still far off. It will involve application of almost every known scientific discipline, for aging is a part of living and life is a complicated enigma. There are so many facets to aging that the thoughtful efforts of all those concerned with the advance of knowledge will be required for the full development of the latent potentialities of later maturity.

The fact that three recent books dealing with different aspects of senescence are being reviewed together emphasizes both the growing interest in this field and its broad scope, for each attacks the subject from a different approach. Two of the volumes are symposia; the third a popular type of presentation of what aging means to the individual.

In *New Goals For Old Age,* Lawton has collected and organized a splendid group of deeply sensitive discussions of the major psychological problems of senescence. The papers were originally delivered in a course, "Mental Health in Old Age," conducted under the auspices of the Section on the Care of the Aged of the Welfare Council of New York City. The lectures were planned to be as practical as possible. Lawton's careful and inspired editing has brought the divergent viewpoints together to create a delightful and decidedly thought-provoking book. Worthy of special emphasis, because of their exceptional merit, are the discussions of Lewellys F. Barker on "Physical Changes in Old Age and Their Effects upon Mental Attitudes," Lawrence K. Frank's on "The Older Person in the Changing Social Scene," and George Lawton's fundamental consideration of "Aging Mental Abilities and their Preservation." Throughout, the book is scientifically accurate and fully documented but it is also so palatable and digestible that there is sheer pleasure as well as profit in study thereof.

The second volume of the three is a monograph intended for "popular" consumption, and as such is an excellent résumé of most of the major problems likely to beset us as we pass from later maturity into final senility. Doctor Gumpert divides his material into four major divisions: (1) The Biology of Aging; (2) Normal Old Age; (3) Social Problems of Aging; and (4) Diseases in Old Age. The first of these is very ably presented. The last section, dealing with the frequent and disabling diseases of later years, suffers from unfortunate oversimplification. Certainly a description of the mechanisms, significance, outlook, and logic of therapy of hypertensive disease in two pages or a discussion of prostatic disorders in three small pages requires omission of much that is pertinent and significant. Such excessive condensation may confuse rather than enlighten the previously wholly uninformed layman. A little knowledge can be dangerous even though that be accurate. Doctor Gumpert is to be congratulated upon the splendid accuracy of his presentation, though throughout the text there runs the same overly optimistic vein which is so apparent in his title. It is all very well to try to make senescence attractive, but people will still dislike and fear the thought of growing old. Sharp realism, with equal emphasis of

the liabilities of aging and the assets of continued maturation is what we need even if it isn't what we want.

The Series of Biological Symposia are surely well known to the great majority of readers of THE SCIENTIFIC MONTHLY. The eleventh volume of the series, *Ageing and Degenerative Diseases,* fresh off the presses of Jaques Cattell, contains the papers presented at a symposium in St. Louis, March 24 and 25, 1944, held under the joint sponsorship of the Research Unit of the St. Louis City Infirmary and the Washington University School of Medicine. The ramifications of gerontology and geriatrics are so extensive that only a few subjects could be considered in the two-day program; thirteen papers and a round-table discussion on aging and the degenerative disorders so commonly associated with senescence from the divergent viewpoints of experimental morphology, biochemistry, nutrition, endocrinology, and clinical medicine. We shall not single out any one of these contributions for particular comment; all are noteworthy for original, sound, scientific thinking. The volume contributes significantly to the advancement of the science of gerontology. It deserves close study by everyone interested in aging.

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A SCIENTIST LOOKS AT FREEDOM

Freedom Is More Than a Word. Marshall Field. 190 pp. 1945. $2.50. University of Chicago Press.

To the man of science, freedom is something more than a desirable and gratifying attainment; it is an absolute necessity. Only in an atmosphere of freedom can science flourish. Without its invigorating presence, the scientific method of experiment, exploration, and adventure inevitably fails. No wonder that many scientists are at this moment displaying such deep concern in the sociopolitical trends of the day. Beyond question, all scientists have a tremendous stake in the plans for freedom that must be put into effect if science is to continue to flourish when the political state necessarily becomes the social service state. This concise, closely printed, slender volume may well be recommended as helpful reading to any man of science who recognizes the fact that for him as scientist, even as for him as citizen, freedom must be more than a word.

In spite of its dimensions, this is really two books in one. First, there is a dissertation on "the proper relationship of the individual to the state and of the state to the individual." The reader who is familiar with the extensive literature in this field, or even those who have read only two or three of the score of books published in the last three or four years that touch upon it, will find little that is new. Many of the phrases will be recognized as old friends. It is, however, an excellent presentation of a social philosophy that appeals strongly to every mind habituated to scientific ways of thought. Never trite, seldom superficial, its ideas move swiftly, logically forward from a description of a society of free men to an analysis of freedom of expression and a critique of the obstacles to its universal development.

This part of the book could have been written by any one of a thousand intelligent, thoughtful surveyors of the American social scene. Its significance stems not so much from what is said as from who it is that says it. Marshall Field is the grandson of the "merchant prince" of the same name who made his fortune in Chicago and London. Perhaps my appraisal of the book's significance is colored by the fact that when I was an impecunious college student I worked for a time as a shoe salesman in the basement of his Chicago store. In any event, it is something of a phenomenon when the scion of a wealthy family, educated at Eton and Cambridge, reveals a social philosophy so completely antithetical to that of the "Old School Tie" and the "playboy." Evidently the "American dream" has vitality and power at the top as well as at the bottom of the social ladder.

The second part of the book is more specific. Only Marshall Field himself could have written it, but here the reader's interest is riveted to the printed pages; he does not care who the author is, nor what his background may have been. It is an account of the objectives and vicissitudes of Mr. Field's two well-known adventures in the newspaper

world, *PM* in New York and the *Sun* in Chicago. The inside story of these efforts to develop freedom of expression in a field restricted by intrenched monopoly is in very truth an epic of our times. It includes, of course, an account of the long and complicated legal battle with the Associated Press, but it was written too soon to end with the final note of triumph that was sounded on June 18, 1945, when the U. S. Supreme Court sustained the injunction, issued by a federal court in New York City last year, ordering the news agency to abandon a by-law allowing competitors to influence the election of new members. At last it is established that the government has an obligation under the First Amendment not merely to refrain from interfering with the freedom of the press but also to protect it actively from private restraints.

Realizing as every scientist should, that the freedom to undertake research is inextricably interwoven with all other aspects of the life of freedom, that freedom of expression is just as priceless a treasure for the investigator in the laboratory as for the reporter in the newspaper office, we may well be grateful to Marshall Field for his valiant battle and for this clear-cut exposition of his philosophy and experience.

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AMERICA THROUGH FRENCH EYES

Democracy in America. Alexis de Tocqueville (Phillips Bradley, Ed.). 2 Vols. 973 pp. 1945. $6.00. Alfred A. Knopf, New York.

IN May 1831 a twenty-six-year-old Frenchman and a congenial companion, Gustave de Beaumont, landed in New York City. Ostensibly they had come to make a study of American prisons for the French Minister of the Interior. In nine months they traveled 7000 miles observing, inquiring, and recording a vast quantity of notes. Out of these data was to emerge *Democracy in America,* "perhaps the greatest work ever written on one country by a citizen of another," as Harold Laski put it in an introduction to the edition under review. Incidentally, contemporary Americans were gratified over a European visitor who come seeking to understand rather than to ridicule.

The tumult of the July Revolution in France had only recently subsided when Tocqueville arrived here in the midst of what American historians have since termed the Jacksonian Revolution. The prolonged depression of the 1830's had served to accelerate a developing social ferment in the American masses. Convinced that the old ruling class had betrayed their interests, the people seized the reins of government and converted the theory of popular sovereignty into an established fact. Tocqueville had dropped into the midst of the bustling generation that was planting in young Abraham Lincoln's mind the enduring ideology eventually to be epitomized in the classic phrases of the Gettysburg address. "The American people reign in the American political world as the Deity in the universe," observed Tocqueville. "They are the cause and aim of all things; everything comes from them and everything is absorbed in them."

American Democracy falls in the tradition of Montesquieu, the philosopher of causation, who persistently sought among social conditions and forces the mainsprings of political action. Fearful of the glorious cloud land of the a priori, Tocqueville endeavored to keep his feet on the solid earth by reveling in the copious data he had gathered. Whatever its shortcomings may be, he produced in the 1830's what at the end of the century was pronounced by a competent authority "the best philosophical discussion of democracy using the United States as an example that can be found in any language."

The key to this commentary centers in Tocqueville's belief, as expressed so well in the preface to the twelfth edition, "that the advent of democracy as a governing power in world affairs, universal and irresistible, is at hand." He had come to study the world's greatest functioning experiment in democracy, hoping to discover a universal guide by which every people could achieve the advantages of such a way of life and yet avoid its defects. So what would happen to Europe and particularly to his own France when this phenomenon crossed the Atlantic was never out of his mind. He succeeded in producing

what Phillips Bradley, the editor of the present edition, considers "a litmus paper by which to test the process of change from aristocratic to democratic institutions."

The reader can get some idea of the scope of Tocqueville's curiosity when informed that he dealt with American geography, social conditions, local and state government, judicial power, the Federal Constitution, parties, civil liberties, unlimited powers of majorities and the checks on their tyranny, the future of democracy, and its influence on the intellect, feelings, manners and on political society.

This French savant was struck with the fact that the United States provided the world's first opportunity to know intimately the infancy of a nation. Americans had somehow hit upon the corrective for centralization of administration by the participation of the people in the process of government, voting for public officials, and creating and implementing public policies, all of which, incidentally, constituted a potent educative force. Though not especially religious himself, he pleaded for the maintenance of faith as man's most powerful dynamic. "I should not hesitate to decide" he declared, "that the community would run less risk of being brutalized by believing that the soul of man will pass into the carcass of a hog than by believing that man is nothing at all."

Who would have expected in the infancy of the industrial revolution and a quarter of a century before the publication of *Das Kapital* such a clairvoyant flash as this? "The manufacturing aristocracy of our age first impoverishes and debases the men who serve it and then abandons them to be supported by the charity of the public." Here is no propaganda based on a petrified ideology but the mature judgment of one whose parents perished by the guillotine in the Reign of Terror. How refreshing to find a century-old faith in democracy as the ultimate way of life for all peoples and the one sure answer to totalitarian materialism.

Whoever would decide to cancel out of this political classic all that the course of a century has outmoded might be astonished at how little he had discarded. American readers owe thanks to Mr. Knopf for making again available a work so long out of print. Typographically it has been executed in the manner book lovers have come to expect in a Borzoi book. Phillips Bradley is to be congratulated on the diligence and care with which this new edition has been edited.

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WHAT IS LIFE?

What Is Life. Erwin Schrödinger. 91 pp. Illus. 1945. $1.75. Cambridge University Press, London.

"WHAT IS LIFE?" is an old and a perennial question. In this little volume an able physicist tries to advance the factual answer to the question by illuminating living matter by the light of the known physics and the known chemistry of dead matter. The results are both instructive and thought-provoking. But the volume brings out little that is fundamentally new to the present generation of biologists. The workers in functional biology have in the last fifty years added significant support to the working hypotheses that the laws of physics and chemistry apply to living matter. However, Professor Schrödinger's little book is intended for laymen. It is, indeed, an aid to adult education. But, as with all real education, even educated laymen will get little from Schrödinger's essay by reading it as a "pastime," for it stands at the opposite pole from the current "spawn of the press and the gossip of the hour." On the first 86 pages (chapters 1–7) Dr. Schrödinger is clearly a physicist and a philosopher disciplined in science, with a good grasp and understanding of both structural and dynamic biology, even though some of his statements or presentations of known biological facts and relations assume knowledge and understanding not yet at hand or concepts less complex than the known facts warrant. Nevertheless, the informed reader is not a little startled to find in the *Epilogue* (pp. 87–91): "On Determinism and Free Will," that Dr. Schrödinger, like some other fellow physicists, becomes an out-and-out religious mystic. I quote: "Let us see whether we cannot draw the correct, noncontradictory conclusions from the following two premises:

(1) "My body functions as a pure mechanism according to the laws of nature.

(2) "Yet I know, by incontrovertible direct experience, that I am directing its (the body's) motions, of which I foresee the effects.

"The only possible inference from these two facts is, I think, that I am the person, if any, who controls the motions of the atoms according to the laws of nature. . . . Please consider whether the above inference is not the closest a biologist can get to proving God and immortality at one stroke. . . . I have become God."

To this reviewer, and I think to most informed biologists, the second premise is obscure, naive, incomplete, and hence essentially untrue. Irrespective of logic, the conclusion drawn is a *non sequitur* in our known biology of man.

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MAMMALS OF THE PACIFIC ISLANDS

Mammals of the Pacific World. T. D. Carter, J. E. Hill, and G. H. H. Tate. 227 pp. Illus. 1945. $3.00. The Macmillan Co., New York.

WHEN the war focussed American attention on the Pacific Islands, professional biologists and scientists in other fields as well had reason to be chagrined at the dearth of informative books on the plants and animals, races of mankind, and general geography of the region. A flood of letters and telephone calls came to museums and libraries and university departments inquiring for references on the natural history of the Solomon Islands, or of New Guinea, or of the South Seas in general. It was disappointing to have to reply that the literature in question is scattered through hundreds of journals, is mostly too technical for anyone but a specialist, and that no syntheses of such information, suitable for the general reader, had been made.

Regional handbooks of the sort desired are a by-product of descriptive botany and zoology, and these departments of biology have suffered from two generations of neglect in American colleges and universities. It is unfortunate that the general public, and a considerable section of the scientific public, does not appreciate the fact that except for such conspicuous creatures as birds and butterflies the exploration of the world for plants and animals is far from completed. The project for an adequate description of the life of the world is a noble one, and one of far-reaching importance to education on one hand and to the progress of research in the related fields of biology on the other.

The Pacific World Series took shape in answer to the specific need for information for the men in the armed forces in their suddenly novel surroundings, and to the equally legitimate questions of their relatives and friends at home. The reviewer justifies a somewhat parental interest in this series by his modest share in the preparation of the introductory volume, *The Pacific World.* It is gratifying to have the successive volumes available, though the course of the war has left many of the islands in a backwash. Perhaps the bored GI stranded on such a forgotten island may need them more than ever.

The present volume, the third to appear out of the projected seven, deals with the mammals in systematic order, beginning with the egg-laying duck-bill and spiny anteater of Australia and ending with the tapirs (the monkeys and apes, according to modern arrangement, appear between the bats and the flesheaters). An introductory chapter explains the system of scientific naming and gives a general idea of how mammals are classified. The chapters on conservation and on the distribution of mammals in the Pacific are especially useful since the Pacific area includes the extremely distinct mammalian faunas of Malaysia, Australia and New Guinea, New Zealand, and the more eastern multitude of smaller islands in Melanesia, Micronesia, and Polynesia. A completely nonzoological GI might bog down in a complicated group like the bats, but will find identification enormously simplified by reference to the concluding section, which gives lists of the mammals known from each island or island group.

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COMMENTS AND CRITICISMS

ON THE WELTFISHIAN OATH

The Scientific Spirit and Human Prejudice

In the September SCIENTIFIC MONTHLY under the title "Science and Prejudice" an anthropologist called the scientist to task for lack of social responsibility. Miss Weltfish's article is commendable for its recognition of personal responsibility for world order, and it has the further merit of making a simple, specific suggestion. But is her evaluation of the scientist and his position in society just, and is her suggestion practicable?

Miss Weltfish illustrates her point by a lengthy description of the activities and personal animosities of anthropologists of seventy-five years ago. This is an odd example to choose because the threat of destruction which we now contemplate does not stem from the researches of the anthropologist; moreover, his methods and principles are not necessarily representative of those of the physical sciences because his is a very young discipline. It would have been more pertinent to consider the social consciousness of a physicist or a chemist. Miss Weltfish might have asked, for example, whether Nobel, when he discovered how to make nitroglycerine safe for the user by mixing it with a porous, inert material, was motivated by purely academic interests or whether he weighed the possibilities of destruction which his discovery would place in man's hands against the undreamed of feats of constructive engineering that it held out. The five prizes for which he left his fortune are some measure of his concern, and it is not a reflection upon him or his profession that on more than one occasion there has been no candidate considered worthy of the peace prize. Again one's thoughts turn naturally to the Curies whose researches form a link in the chain that led to the production of the atomic bomb. We can never forget that when the significance of their recovery of radium from pitchblende was recognized they unhesitatingly dismissed the thought of patenting the process, considering it contrary to the scientific spirit to withhold for personal profit a discovery which would be of untold benefit to mankind. In recalling the character of great scientists, I do not mean, however, to employ the fallacious method of supporting a thesis by individual cases. We know that, as among all men so among scientists, there are those who are driven by selfish ambition as well as those whose devotion to high purpose is worthy of a saint. But if we are to think clearly on this question we must not confuse the spirit of inquiry, which is inseparable from the sciences, with the foibles displayed by individual scientists, not because they are scientists but because they are human. The common misconception of the man of research as characteristically preoccupied with the selfish satisfaction of his curiosity can be corrected by a review of the history of science in the light of its interplay with industrial development, systems of philosophy, and social, economic, and political theory. Perhaps, however, the most immediate and effective answer to those who regard the scientist as indifferent to the consequences of his discoveries is afforded by the stand which scientists are today taking on the use of the atomic bomb.

The oath which Miss Weltfish proposes is an appeal to idealism, to the desire of the individual to live as a useful, integral part of common humanity. But an ideal which cannot find expression in action is only the good intention that paves the way to hell. The question is, how would the oath affect the activities of scientists, and what security, if any, would it give us? Suppose the men who worked on the atomic bomb had taken it; how could they, knowing the threat of German development of such a weapon of destruction, have refused their government's call? While hazards exist, is it wrong to provide for self defense? If not, then the possibility of the offensive use of the same instruments confronts us. An even more serious objection is that the oath is based upon the naive assumption that research can be separated into two classes, beneficial and harmful. Nobel's dynamite affords a simple illustration of the fact that a discovery may be used either constructively or destructively depending not upon the intent of the scientist but upon the moral status of mankind. It is indeed curious that the scientist should be singled out as a scapegoat upon whose head the sins of society are placed.

Discoveries of fundamental theoretical importance often have the most revolutionary practical applications; consequently, every scientist well knows that if we were to subtract from our knowledge everything which can be put to harmful use, the tattered remnant would hardly be recognizable. Moreover, it is seldom if ever possible to foresee the full results of a discovery. James Watt was an instrument maker at Glasgow College when the task of repairing a defective model of a Newcomen engine used for class demonstrations caused him to note its large consumption of steam. Even though he was thinking of industrial applications when he bent his ingenuity to its improvement, it would hardly be reasonable to demand that he foresee the multitude of problems arising from the social revolution to which it contributed so much. Faraday's interest in

the relationship of electricity and magnetism was more theoretical. After the effect of an electric current on a magnet was observed, the question of the reverse action was on his mind and the subject of experiment for seven years before his study of effect previously neglected enabled him to formulate the laws of electromagnetic induction in ten days. Would Miss Weltfish becloud our appreciation of Faraday's genius by suggesting that, in as much as he did not anticipate all the consequences of the use of the electric motor, he was not giving due consideration to "the purposes for which his investigation was being carried out. . . ."?

Whether the scientist is concentrating his energies upon the solution of an industrial problem or trying to gain a clearer understanding of natural phenomena, his discoveries may sooner or later call for social adjustment. It is not within his power to decide whether that adjustment will be wise and progressive or unthinking and disastrous. Consequently, discoveries in the natural sciences demand compensating advances in the social sciences, in politics, economics, and morals. If we fail to make those advances, it is a sorry expedient to tell the scientist that he must be a good boy and not discover anything we can misuse.

Aside from being impracticable and misdirected, a movement for a scientist's oath might well have the effect of hindering the accomplishment of its ultimate purpose in so far as it absorbs effort and diverts attention from the problem of understanding the causes of social and international maladjustments. One is reminded of Bishop Ripon. When in 1927 he pointed out the fearful consequences of the lack of balance in our culture, he put his finger on a weakness that has worried many, but his proffered solution, a moratorium on science, suggests a teacher demanding that his capable pupils stop studying in order to let the backward ones catch up. Our pressing need is to accelerate progress in the social sciences. We plead complexity of subject matter as the cause for their retardation relative to the physical sciences; but it is not a foregone conclusion that this is the sole cause. Nor would anyone argue that the rapid advance of the physical sciences is due to simplicity of subject matter. The significant circumstance is that our material environment affords a realm where it has been easier for man to detach himself from his likes and dislikes, his hopes and fears. It is not an accident that in great scientists we find a recurring pattern of traits with thirst for understanding, the highest degree of mental integrity, humility, and ingenuity conspicuous. These traits are not peculiar to the scientist, far from it, but he is distinguished by the degree to which they are developed. He gives us a new realization of what mental integrity means. Both experiment and mathematical computation make him keenly aware of the possibility of error in observation and conclusion, of the insufficiency of the usual accumulation of data, of the risk of neglecting some of the many factors influencing his results, and of the danger of being blinded by preconceived ideas. Hence his humility. But he also has confidence and determination because he knows from experience how far ingenuity—imagination and originality, if you will,—can take him. Miss Weltfish is perhaps correct when she says that it is little understood that science is an instrument for seeking the truth, and we might add, the quality and effectiveness of that instrument is not appreciated outside the sciences.

It is not easy to study man with the same detachment with which the physicist studies matter because as soon as we consider human relations we enter the "field of force" of ideas about what is desirable. The recognition of this fact should steel our determination to maintain the scientific spirit in all research. Throughout time men have fought savage and disastrous battles over ideas; ideas advanced more often than not with talk of betterment. But the earnest desire to understand, eagerness to extend knowledge, to put it to test, the honesty to admit error, the courage to recognize insufficiency of facts and experience, these are peacemakers and they are of the essence of science.

At this particular moment in history we are faced with the question of how to develop mutual understanding in a world made physically one before we were psychologically prepared for it. The anthropologist made a significant contribution when he demonstrated the closeness of biological relationship of all races of man, although blood relationship alone does not assure harmony of life. The similarities of man's cultural aptitudes are even more encouraging, and when the mind runs over man's activities, the sciences stand out as particularly adapted to foster the spirit of internationalism. Our body of scientific knowledge is a monument to mankind which is entirely free from national or racial flavor. If we are not keenly aware of this and tend to think with particular pride of the contribution of our fellow countrymen, it is the fault of our education. By contrast, linguistic differences set men apart, increase the difficulty of understanding. The arts leap the bounds of language and constitute a heritage which enriches all mankind, but they also often draw inspiration from and express nationalistic feelings; we can identify the artistic styles of different peoples, and independent creative genius looms large. Certain ethical principles are shared by great religious systems, but we must contend with intolerance which has often made it impossible for people of different religions to work together for the attainment of their ideals. The sciences on the other hand open man's

mind to an intellectual comradeship of the ages that knows no bounds. They have grown as a single interrelated and interdependent whole. Everyone must thrill to the realization of how ideas and obsrvations of men of far countries have repeatedly interlocked and stimulated, leading to ever broader and truer understanding. We can pick up the thread anywhere and follow its intricate weave. Take current electricity, for example. There was the Italian Galvani noting that a frog's leg contracted under the effect of a discharge from an electrostatic machine and that the same reaction occurred when a nerve and muscle were connected by different metals. His countryman Volta demonstrated that the essential phenomenon did not depend on animal substance and built a voltaic pile, producing electric current, which was quickly used in studies of the chemical effects of current by Englishmen, including Humphry Davy and, finally, Faraday. Then Oersted in Copenhagen noted the effects of current on a magnet, the importance of which was immediately appreciated by a Frenchman, Ampère, who studied the laws of the forces acting between currents. The Germans, Ohm, Gauss, Weber, speeded progress by exact definitions of quantities and invention of a system of units for electrical measurement. Faraday was aided in his investigation of induction by the improved electromagnet designed by the American Joseph Henry; and so on in an unending stream. Those were days of limited communication. As we approach our own times, reactions quicken, and the circle widens to include the world. One name, Noguchi, should suffice to remind us that we owe our debt of gratitude and respect to scientists of all races and all nations.

Contemplating this aspect of science, we seem to be faced by a paradox because in this age of outstanding scientific achievement the sense of nationalism is quickened and racial prejudice flares. Ours, however, can be called a scientific age only if we measure it by the achievement of the scientist himself and by our dependence upon him for our material comforts and pleasures. One need only reflect upon our approach to pressing domestic and foreign problems to realize how far we are from having attained what might properly be called a scientific state. One of the tragedies of this time of cataclysmic change is the extreme difficulty of learning the truth about what is going on in the world. We are condemned to struggle almost hopelessly with a deluge of opinion and partial information highly colored by prejudice, fear, ambition, and even good intention. Suppose foreign diplomats or leaders of labor and industry could meet with the same mental integrity that the scientist must exercise in his research, how different our world would be! Madame Curie once said, "Nothing in life is to be feared. It is only to be understood." It would do no good to extract an oath of mental integrity from those entrusted with the direction of our affairs. We must begin farther back and learn to understand what Faraday meant by the simple maxim "Keep thyself teachable." In other words, we face a tremendous educational problem, and I believe the brunt of responsibility should be borne by the social scientist. Therefore, as Miss Weltfish's fellow professional, I would plead with her that it is not for us to accuse the scientist; rather we should learn to understand the spirit of his research in order that we may fulfill better our duty to society.—ANNA O. SHEPARD.

Ethical Dualism

Dr. Weltfish is to be commended for her paper entitled "Science and Prejudice," and for her good intentions expressed in it. However, it appears to me that in her efforts to bar the "ivory tower escape" for the scientist, Dr. Weltfish is entirely unaware of what I shall term the "ethical dualism" of matter.

To explain my statement by way of a specific example let us consider two chemists. One works on explosives such as TNT, and the other on better aviation fuels. If the products of their efforts are used in war, to fill shells or to power bombers, the chemists have violated their pledge. If on the other hand the explosive was used in quarries and mines, and the fuel for air transport, travel, and exploration, they have worked toward the good of humanity. Many similar cases could be cited.

In my opinion, Miss Weltfish does not take into account that the scientist is not at the dispensing end, as, e.g., the physician, but that he is a creator of matter, of processes, of machines which per se have no ethical values. Those who control their application and use bring in the ethical problem and can turn them into benefit or destruction of humanity.—FREDERICK C. NACHOD.

Prejudice against Prejudice

A Weltfishian oath for scientists! Why would it be that I could mistrust the integrity of the scientist who took this oath, provided he took it with conviction and vehemence and not merely in the impetuosity of an emotional moment? Even the most ivory-towered of scientists must want "the good of humanity," must rue "the destructive forces of the world." Perhaps the difficulty comes in the oath's condemnation of "th ruthless intent of men." Doesn't Dr. Weltfish want us to be ruthless against what is sometimes called "intolerance"? Isn't she fomenting ruthlessness against ruthlessness, hoping to get us to swear to this paradox?

It is not as if I had not puzzled and written about this problem of scientific prejudice for the last

twenty years. I have. But there is an essential contradiction involved, a contradiction inherent in human nature. Even Dr. Weltfish's article illustrates it. She complains about how Quatrefrages was bombed out of his ivory tower and started up some social harm while he was out, and then about how modern scientists too often withdraw into the tower, leaving outside enough particular responsibilities to do a lot of social harm. It is not a question of being in the tower or out of it. You can do both good and harm in both places. Dr. Weltfish has her eye on the harms.

The basic dilemma is here, I am sure, the dilemma between drive and prejudice. It is thus the dilemma of ego-involvement. Science has its equivalent of the Hippocratic oath, although the formula has never been set down. The scientist should follow through to the truth, should read the book of nature accurately no matter how unpleasant the reading may be. Such an oath he might take, paradoxical as it is, for it is an oath to be prejudiced against prejudice. He might take it, but, because of the paradox in it, he could never wholly fulfill it. To fight prejudice takes a prejudice against prejudice. To do research you need an hypothesis to test, and the man who holds his hypothesis so lightly that its disproof is just as pleasant to him as its verification is not the man who works long hours for little pay and limited social recognition. Enthusiasm is prejudice. That's the way human motivation is constituted.

The successful scientist has really got to be a schizophrene. He has got to get all excited about an hypothesis that he thinks might be true (and that means he hopes it is true) and then be ready to celebrate because he finds it is false. No wonder he goes wrong. No wonder we keep meeting silly scientific controversies whose chief purpose seems to be to prove one or two protagonists right. No wonder the scientific truth has so often to transcend the individual, waiting upon his professional posterity, because the individual cannot himself transcend his own pride and ego-involvement.

The conventional way of meeting this problem is to provide the ethic of judicious disinterestedness. The scientist constrains his public behavior in accordance with the ethic, keeping his private hopes and prejudices to himself and to his friends—except, of course, when his reasons turn out to be rationalizations and his cat is out of his bag. That's what happened to Quatrefrages.

On top of this perpetual dilemma Dr. Weltfish wishes to impose another prejudice—another prejudgment. She wants the scientist to decide, not only whether his own egoism is filtered out of his publication, but also whether his finding is good for society. How is he to know what is good without knowing what the results of discovery will be? I do not speak of the obvious. Of course, the psychologist who has finished testing a schoolchild in the interests of impersonal truth does not murder the child and throw him away. He gives him a cracker, pats his head, and sends him home. The cracker is not science but what decent people do. But what about splitting atoms? Will that do more harm than good? Suppose those physicists had taken a vow against "the destructive forces of the world"? But perhaps it is not fair to put that difficult question up to Dr. Weltfish.

Here, however, are some simpler questions. Should psychologists and anthropologists continue to investigate the nature of psychological defense and escape mechanisms that the psychoanalysts have given us? Should they publish their results? Should they disseminate them widely, trying to get ordinary people to think scientifically about human nature? It is argued that such activities do social harm because they tend to induce in men the habit of thinking about their behavior as the consequence of external causes and past events. That, it is held, makes them irresponsible. Read R. B. Perry's *Puritanism and Democracy* and then decide whether anthropology and psychology with their support of the un-Puritanic irresponsibility of psychic determinism are or are not adding to "the destructive forces of the world."

In short, I feel that the aggressively socially minded psychologists are just one kind of psychologist, a bold kind, who venture to prejudge what in science will be good and what bad, who probably seldom suppress the truth for humanity's sake, but who probably win the battle against their own ego less often than their colleagues who feel less intensely the problem of making science come out the right way. And what goes for the psychologists goes for the anthropologists and the other scientists too.—EDWIN G. BORING.

The Last Word

In connection with Professor Boring's criticism of the scientific oath I propose in the September MONTHLY I should like to make some general remarks and then be specific to his point:

In the course of many discussions, criticisms of the oath have been of two main sorts: *one*, that it should treat of the integrity of the scientist in seeking the truth, and *second*, that it will not accomplish the purpose that it is apparently designed to achieve.

With reference to the first objection, I want to point out that in the body of the article "Science and Prejudice," I have said that for the most part the scientist has done an excellent job along these lines. I do not feel, therefore, that an oath for this purpose is now needed.

The whole object of my proposal is that there is another aspect of the scientist's experience that re-

ceives from him either very meager consideration, or none at all; viz., the purposes for which his work is carried on, and his place in society and his obligations to it. It is to the latter emphasis that the oath is addressed.

On the second question as to whether the oath will accomplish its purpose:

It is true that the fact that a scientist has taken this oath is no guarantee that he will automatically acquire a social consciousness, and that, further, this social consciousness will take a "good" direction. It is, in fact, quite possible that a scientist might go on doing precisely what he has done before, even after taking the oath.

I think there is a rather general human desire to get the world's work done by magical formulas. We share this desire with the New Guinea native and with many other human beings who, by reciting a spell, would call down the aid of the supernatural powers to alter the whole course of their lives. We like to think that by finding a four-leaf clover we can change the whole atmosphere of the universe so that it is especially shaped to the desires and needs of one person; viz., ourselves.

On a reality level, one could not possibly devise an oath that would completely change the "moral atmosphere" of a scientist.

My particular purpose in proposing this oath is twofold: to stimulate the young science graduate to consider his place in society and to get the people themselves constantly to consider and reconsider the place of the scientist in society. In other words, I do not expect the scientist to gain a social conscience by a single flash of insight nor by lonely introspection, but with the collaboration of the people of whom he is a part. To me this sort of procedure is the essence of democracy. The fear of the scientist that has arisen in the public as a result of the atomic bomb is a rather discouraging symptom of the aloofness of the scientist.

Professor Boring says that he would be inclined to distrust the scientist who sincerely took such an oath. I think he is going too far in the opposite direction, in the same sense as those who expect the recitation of an oath to have immediate and universal positive effects.

Professor Boring's reasoning on a psychological level is extremely interesting. He feels that it is a common human tendency to "put your money" emotionally on one or another side of a question, and that for your side to win is an ego-satisfaction while the opposite is a threat or source of displeasure. He pictures the quest for objectivity on the part of the scientist as an inhibition of this satisfaction because he must act as if he didn't care whether his results went with or against his hypothesis. My teacher, Franz Boas, was never through warning us that a negative result was as valuable scientifically as a positive one. He repeated it many times in the hope that we would follow it unfailingly. There is no doubt that it is a hard thing to do.

Now Professor Boring maintains that this is enough of an emotional strain on the scientist without demanding of him that he also examine the social implications of his work. On the other hand, I believe that the process of understanding the social milieu in which he lives and of considering the place of his work within it will mitigate his anxiety at negative results rather than augmenting it. The value of his results, positive or negative, will be much clearer to him than if he considers them in introspective isolation.

Also, I would not demand of the scientist that he try to predict all the results of his scientific discoveries before he made them known. This is something he certainly could not do. What I would ask of the scientist is that he know and understand his society well enough to have some gauge of what his work means in it. In other words, it would be of great value to his work for the scientist to be a citizen in the full sense of the word. (See my article "The Scientist is a Citizen," *Christian Register*, September, 1944, vol. 123, no. 9; pp. 325–7.)

At the risk of being anticlimactic, I want to add that there are those who would defend the social isolation of the scientist on the grounds that he doesn't have time to gain information about the social scene—that he is necessarily too preoccupied with his work. This concept of the scientist as incessantly preoccupied does not come entirely from the exigencies of his work. Projective thinking does seem to demand irregular hours, times of long and intensive preoccupation, and times of inactivity. But this does not mean that in the periods of inactivity other matters cannot be considered.

Because of this irregularity in the working time of the scientist, science has been conceived as a kind of random procedure awaiting only inspiration—which it is not. The procedures of science are certainly orderly and predictable. The scientist is creative precisely because he follows these time-honored procedures. And even the problems that appeal and suggest themselves to the scientist are not random, but have a continuity in the history of science and are a response on the part of the scientist to contemporary social stimuli.

In suggesting that the scientist give some of his attention to this social scene, I am suggesting that in common with many fields of activity in modern civilization, specialization has proceeded too far and that it is desirable to seek a greater degree of integration. Professor Black, also in the September issue of the MONTHLY, writes to this point in his

article "A Lend-Lease Program for Philosophy and Science" in which he points out that historically science was not as limited to technical procedure as it is today and indicates that philosophy be reintegrated with science. I am, of course, fully in accord with this concept—and I would advocate this course of action as well as the one I am suggesting.

In these times of transition, the scientist must outgrow his role of alchemist, astrologer, and esoteric medicine man and become a worker among the people —albeit a highly skilled worker with his brain.—GENE WELTFISH.

Beachcombing

The article "Scientific Beachcombing" in the October SM represents the opinions of the author but does not give the cause, why the Santa Barbara shoreline is scalloped. Enclosed are four photographs of water being poured from a widemouthed graduate. Surface tension causes the drawing together of the stream, as shown by the reduced cross section. In the same manner the backwash from the wave front draws together and breaks up into scallops. The increased erosion along the center of the scallops leaves the fringe of points. This fringe is an effect of which surface tension is the cause.—HIRAM W. HIXON.

Dr. Evans' short article "Scientific Beachcombing" in the October issue is interesting in view of the fact that so little scientific study of beach forms is available. My late brother, William F. Jones, at one time Associate Professor of Geology at M.I.T., was keenly interested in beach formation as a practical matter in connection with the control of erosion. He did an enormous amount of work in the detail survey of beaches, both stable and unstable. At one time he sent me a set of contours of stable beaches. These proved to be parabolas of one family, the beach crest being under the focus. So far as I could make out from the available data, the particular parabola at any locality depended on the average size of the sand (or gravel) particle, the distance between mean high and mean low water, and on the average size of the breakers.

This conclusion is largely speculation. The shape of the contours is not speculation. Possibly Dr. Evans may be interested enough to either verify or disprove the above. If correct, it has an important bearing on the use of artificially stabilized beaches in coast protection, a result that has not been attained by the use of groins, jetties, bulkheads, etc.—methods that futilely resist rather than dissipate the energy of the waves, and seek to prevent natural long-shore sand movement.—BASSETT JONES.

Melly Clismas

So Association want big building? Well, easy if big shot give big money. Big atom bomb make big noise and stop big war and big shot got big money to make building. Government also got big money, 3 million carloads gold in big cave in Kentucky. Let government give big building, so cost nobody anything. Let government do everything so everybody have no trouble and pocketful of money.

What for big building anyway? Diogenes great scientist. He live in tub. Confucius big shot thinker have no big building. Mohammed sit on big stone all alone in big desert. Robinson Crusoe sit on island. Jack Horner sit in corner. Big men have no big building, but have big brains. Let somebody else do work and give money make big building. Scientists have big brains, save money, smoke big pipe, take it easy, don't hurry and don't worry. Allee same in million years. Keep kimono on and take it easy.—OLD HI HO.

Our honorable and venerable Hi Ho is great apostle of optimism. He preach gospel of wishful thinking. Wish hard and wish come true. Wish very, very hard for beautiful stone monument building home of A.A.A.S., and, lo and behold, money come flying. Yes? No!! Honorable members A.A.A.S. trained science thinkers; Ho Hum hope have outgrown belief in fairies, Santa Claus, and miracles. Science make miracles like split atom, but Science not wish for them. Science hard work. Hard work means sweat. Sweat great fertilizer all growth. Sweat very, very honorable. Great man Churchill dignify sweat even for British faces: "Blood, sweat and tears."

No argue that new building not needed. But no like wishing. Building belong members, serve members. Members appreciate if all pay alike. No pay, no sweat, no appreciate. All pay same if money come from bigger dues. Science now big. Science now have plenty face. People afraid and no trust science men. Afraid make tools to destroy. Peoples forget tools no destroy, but fools who misuse tools do. Science can do plenty good. To lead need keep face. Science lose face, which very sad, if go begging. Science must know people. That my business.—HO HUM.

Belated Acknowledgment

I would like to express gratitude to Clarence and Elizabeth Ferry of Westwood Village, Los Angeles, for their excellent photographs of artifacts which appeared in my article, "An Anthropological Campaign on Amchitka," in the July 1945 issue, acknowledgment of which was omitted through an error.—PAUL GUGGENHEIM.

THE BROWNSTONE TOWER

ALONE in the Brownstone Tower, I am thinking tonight of my father-in-entomology, Dr. William Moore, who not only introduced me to the Japanese beetle in 1921 but taught me something about natural history in the fields and woods near Riverton, N. J. He was, and is, a robust lover of life. Having a lively imagination and an unrestrained sense of humor, he not only saw the fun in every odd incident but treasured every personal anecdote and, like Mark Twain, embroidered each one with riotous exaggeration till, after a few tellings, the truth became totally eclipsed by fancy.

The story I remember best, of course, concerns myself. The simple truth was this: Needing a supply of tent caterpillar egg masses for experimental work in early spring, Dr. Moore took me, his chemist, out in the field and taught me to recognize on dormant wild cherry trees the conspicuous ringlike bands of eggs laid on twigs by moths the preceding summer. Then he made it clear that he wanted me to climb such trees and procure twigs bearing these varnished egg masses. I viewed this assignment with misgiving because my center of gravity was abnormally low and I had previously flunked my gymnasium examination on chinning. But duty called and, with Dr. Moore looking on, I somehow managed to ascend to the first crotch of the tree. But to reach the twigs I had to get out on a limb. I tried and soon found myself out on a limb in the usual sense of that well-worn phrase. To put it plainly, I was stuck and had to give up my attempt to secure the twigs.

Dr. Moore has told that story off and on ever since, until today it goes something like this: "One warm day in early spring at the Japanese Beetle Laboratory Campbell concluded his lunch with a generous slice of lemon pie. Then we went out to get some tent caterpillar egg masses. I found a good tree and asked Campbell to climb it to get the eggs. He huffed and puffed his way up into the tree and crawled out on a limb. All of a sudden the lemon pie lurched and threw him off balance. Down he swung until he was hanging from the limb like a sloth. He couldn't get up and he couldn't get down. Finally, in order to let him go home, I had to cut the tree down!"

When, fresh from Philadelphia and the University of Pennsylvania, I went to work with Dr. Moore I thought there were only two species of birds in the vicinity: English sparrows and robins. He opened my eyes to the birds of the Riverton golf course and a little later at Rutgers my friend and contributor to THE SCIENTIFIC MONTHLY, Dr. L. A. Hausman, increased my ornithological knowledge, which, however, is still elementary and fragmentary.

Although my birding days are over, my most memorable encounter with a bird occurred only a few weeks ago here in the Brownstone Tower. It was a hot, dark night, and my circular window was wide open and hence horizontal. Perhaps I needed to relax from my work for, without having heard a sound, I raised my head and turned to look at the open window. There, perching on its outer periphery and looking solemnly at me, was an owl—a large white owl, it seemed to me. I did not invite him to come in. I felt that my cubicle was too small for an owl and an editor; that one might get in the other's hair or feathers, as the case might be. So I outstared him, and he turned and flew silently away. Later, with the help of A. A. Potter and Herbert Friedmann, I learned that my feathered friend was a barn owl and that he was a family man. I saw his family flying at dusk and heard them chattering. They undoubtedly lived in one of the smaller vacant towers of the Smithsonian. For further information I turned to Dr. Hausman's *The Owls of New Jersey* and read: "[Its] eerie sort of flight and the habit of nesting in deserted buildings, old towers and steeples, and ghostly ruins have resulted in the bird's being taken more than once for the 'haunt' or spirit of such localities, and more especially since one of the common flight notes is a weird, subdued hissing scream." Then I knew that I had seen the ghost of the Smithsonian and that, if I did not take care, my body would be found one night in *rigor mortis*.

The foregoing owl story is the truth and nothing but the truth. I should like to know what Dr. Moore would make of it. I do know how it has been exaggerated and distorted by that unprincipled poetical prevaricator Paul H. Oehser, whom I introduced to our readers last month. Paul's version of the story follows:

The Owls of the Brownstone Tower

The owl, from ornithologists I've heard,
Is commonly a solitary bird;
It flits around at night *to-whit-to-wooing*
And never cares what other birds are doing.
That's usual. But every now and then
The owl seeks out the company of men,
Consorts with scientists in lofty towers,
As if to emulate their human powers,
Follows them up into their musty attics
Absorbing wisdom, wit, or mathematics.

A story known by every Washingtonian
Concerns the owls that live in the Smithsonian:
One night a Dr. C. at a late hour
Climbed to his office in the Brownstone Tower,
And soon, within his cozy foursquare crypt,
Was lost in scientific manuscript;
When lo! exactly fifteen minutes later
A pair of owls got off the elevator.
In somber style they marched into the room
As serious as a little bride and groom.
"Excuse," spoke one of them, "our innate shyness,
But we would ask a favor of your Highness."
"Shoot!" said the Doctor to the spokesman fowl,
"I'm not a man who would turn down an owl."

Seeing the Doctor's heart so promptly thawing,
The birds proceeded without hem or hawing:
"It's this," they said, "we've tried by every media,
But we can't lift your darned encyclopedia;
Last night our brother nearly killed himself
Trying to get that atlas off the shelf;
And *you* should know how very necessary
Is access to a Webster's Dictionary.
We won't drop pellets in your chair no more
If you will leave them books out on the floor."
The Doctor could but cry, "Well, I'll be blessed!"
And with a smile he quickly acquiesced.

And that is why those owls have grown so wise
Up in their towering Brownstone Paradise;
They live and die and raise their fluffy broods
Immune from usual vicissitudes;
Envied by all the other birds of prey,
They read all night and sleep throughout the day,
And quite emboldened by their self-reliance
Nest in the very periwig of science.

—PAUL H. OEHSER

In January of this year, when The Brownstone Tower first appeared in the SM, I mentioned those members of the A.A.A.S. staff who have something to do with THE SCIENTIFIC MONTHLY. Recently some changes have occurred. My assistant, Mrs. Scovill, recovered from an obscure infection or allergy shortly after Captain Scovill returned from Germany. She left the hospital to join him at Fort Leavenworth, Kansas. She was a loyal, able, and cheerful worker who did much for the SM during her year in the Tower. Her place has been taken by Gladys M. Keener, of Chicago, a woman of experience in editorial work on a national periodical.

We shall also greatly miss Sam Woodley, who performed many services for the SM, including proofreading during my illness. For more than 25 years Sam worked like a superman for the A.A.A.S. I wish that every member of the Association could thank him personally for his devoted services and wish him success and happiness in the less exacting task he has undertaken in private business. Sam's place will be taken in part by Dr. John M. Hutzel, a graduate of the University of Michigan and Ohio State University. Dr. Hutzel, a Lt.j.g., now wears the uniform of the Fourth Marines. Responsible for insect control during the invasion of Iwo Jima, he directed the airplane spraying of the island with DDT. At present he is the Navy's entomologist in Washington and is writing a manuscript for the SM on control of malarial mosquitoes. Upon his release from the Navy, which is pending, he will begin to "get things done" for the Association.

Our advertising manager and book review editor, T. J. Christensen, and his assistant have left the dungeon and moved to temporary quarters on the campus of American University, Massachusetts and Nebraska Avenues, Washington 16, D. C. There Mr. Christensen, who is also advertising manager for *Science,* will work with the new editor of *Science,* Dr. W. L. Valentine, and his staff.

The end of the year approaches with much undone. Many readers have been good enough to write to us about articles that have appeared in the SM, or have expressed their opinions on other matters. Owing to Mrs. Scovill's long illness it was not possible to acknowledge all these letters. Perhaps it would be best to try to discharge these obligations here by thanking those who have written to us and with the New Year make a fresh start on current correspondence. To those who have received Christmas cards from me in the easier days before the war and to my former students I take advantage of The Brownstone Tower to say, "I have not forgotten."—F. L. CAMPBELL.

INDEX

CONTRIBUTORS

Principal Articles

ADOLPH, E. F., The Self-Maintaining Organism, 57
ANDREWS, E. A., R. P. BIGELOW, and T. H. MORGAN, Three at Bimini, 333
ARCHER, W. A. (Translator), The Problem of the Amazon, by F. F. NETTO, I, 33; II, 90

BEKKEDAHL, NORMAN, Brazil's Research for Increased Rubber Production, 199
BENJAMIN, HARRY, Eugen Steinach, 1861–1944: A Life of Research, 427
BERNAL, J. D., Transformation in Science, 474
BIGELOW, R. P., see ANDREWS, E. A.
BLACK, MAX, A Lend-Lease Program for Philosophy and Science, 165
BOGGS, S. W., Mapping Some Effects of Science on Human Relations, 45
BRAND, C. J., and H. R. SMALLEY, The Art and Science of Crop Feeding, 7
BRODY, SAMUEL, Science and Dietary Wisdom, 213

CAIN, S. A., Pic la Rhune, 421
CARLSON, A. J., The Science Core in Liberal Education, 379 (reprinted)
CHICK, HARRIETTE, and ALICE COPPING, Britain's National Bread in Balanced Nutrition, 226
COPPING, ALICE, see CHICK, HARRIETTE
CRISSMAN, PAUL, Causation, Chance, Determinism, and Freedom in Nature, 455

DODGE, E. S., see SPECK, F. G.

EVANS, O. F., Scientific Beachcombing, 245

FENNELL, J. L., The Tropical Grape, 465

GLENDINNING, R. M., Desert Change: A Study of the Boulder Dam Area, 181
GRANT, CHAPMAN, A Biological Explanation of the Carolina Bays, 443
GUDGER, E. W., Is the Giant Catfish, *Silurus glanis*, a Predator on Man? 451
GUGGENHEIM, PAUL, An Anthropological Campaign on Amchitka, 21

HAZLEHURST, T. H., The Logic of the Lost Year, 289
HEDGPETH, J. W., A Century at the Seashore, 194

KARTMAN, LEO, Soviet Genetics and the "Autonomy of Science," 67
KELLER, A. G., The Fallacy of the "Lost Year," 85

LAAS, WILLIAM, An English Picture of the United States, 109
LANDIS, CARNEY, and JANE CUSHMAN, The Relation of National Prohibition to Mental Disease, 469
LEHMAN, H. C., "Intellectual" Versus "Physical" Peak Performance: The Age Factor, 127

MILLS, JOHN, Electrical Communication, 138
MOORE, R. A., Medical Internationalism, 173
MORGAN, T. H., see ANDREWS, E. A.
MYERS, D. M., The Spirit of Truth, 150

NETTO, F. F., The Problem of the Amazon, I, 33; II, 90 (translated by W. A. ARCHER)

OEHSER, P. H., The Fable of the Scientist and the Poet, 378
ORE, OYSTEIN, Our Everyday Reckonings, 372

PARR, A. E., Peace and Justice, 5
PHILLIPS, R. W., Impressions of India, 397
PRESTON, F. W., Freedom of Research, 477

REHN, J. A. G., Man's Uninvited Fellow Traveler—The Cockroach, 265
REYNOLDS, S. R. M., Human Engineering in the Army Air Forces, 277
ROMANELL, PATRICK, A Philosopher's Reply to a Scientist's Ethic, 293

SCHNEIDER, JOSEPH, On the Social and Moral Implications of Science, 353
SHALOWITZ, A. L., Safeguarding Our Seaways—The Modern Nautical Chart, 249
SHAPIRO, H. L., Ethnic Patterns in Latin America, 345
SHAW, E. B., United States Restrictions on Argentine Beef, 101
SKUTCH, A. F., The Naturalist's Dilemma, 361
SMALLEY, H. R., see BRAND, C. J.
SMITH, R. C., Functions and Operations of the National Roster, 287; Biologists in the National Roster, 359
SPECK, FRANK G., and ERNEST S. DODGE, On the Fable of Joe Pye, Indian Herbalist, and Joe Pye Weed, 63
STEERE, W. C., The Cinchona-Bark Industry of South America, 114
SUMNER, F. B., A Biologist Reflects Upon Old Age and Death, 143

URDANG, GEORGE, The Legend of Cinchona, 17

VICTORY, J. F., Keeping America First in the Air, The NACA and Aeronautical Research, 227 (reprinted)
VISHER, S. S., Relative Heating Requirements for American Homes, 313

WALKER, E. H., Natural History in the Armed Forces, a Résumé of Some Recent Literature, Mostly Botanical, of Interest to Servicemen, 307 (Bibliography, 310)
WEISS, H. B., Insect Response to Colors, 51
WELTFISH, GENE, Science and Prejudice, 210 (The Scientist's Oath, 211)

Science on the March

ADOLPH, W. H., Physiological Research in Wartime China, 153
ALBRECHT, W. A., Army University Centers in Europe and the Sciences, 488

BAILEY, M. V., Defoliation of Plants for Profit, 234

CAMPBELL, F. L., Atomic Thunderbolts, 233
CHESTER, K. S., The Genes of Plenty, 483

DAVIS, D. E., The Development of Social Nesting Habits in Cuckoos, 72
DE BRUYN, P. P. H., Characteristics of Locomotion of White Blood Cells, 485

GOODHUE, L. D., Insecticidal Aerosols, 413

JOFFE, A., The Main Problems of Physics in the U.S.S.R., 154

KIRBY, HAROLD, Charles Atwood Kofoid, 415

MEYERHOFF, H. A., The Berlin Conference, 317

WITTWER, S. H., Some Nutritional Aspects of Green Leafy Vegetables, 71

Book Reviews

(Indexed by reviewers)

ATTWOOD, W. W., *The Pacific Islands Handbook, 1944*, by R. W. Robson, 75

BINKLEY, W. E., *Democracy in America*, by Alexis de Tocqueville, edited by Phillips Bradley, 494
BROWNE, C. A., *Discovery of the Elements*, by Mary Elvira Weeks, 326

CALDWELL, O. W., *Education for Responsible Living*, by Wallace B. Donham, 160
CARLSON, A. J., *The Way of an Investigator*, by Walter B. Cannon, 319; *What Is Life?* by Erwin Schrödinger, 495
COGHILL, R. D., *Microbial Antagonisms and Antibiotic Substances*, by Selman A. Waksman, 324

FARNSWORTH, H. C., *Food for the World*, edited by Theodore W. Schultz, 322
FENTON, W. N., *The Science of Man in the World Crisis*, edited by Ralph Linton, 386
FITZPATRICK, H. M., *A Manual of Soil Fungi*, by Joseph C. Gilman, 238

GRAUBARD, MARK, *Tempo and Mode in Evolution*, by George Gaylord Simpson, 76

HALLOWELL, A. I., *The Psychological Frontiers of Society*, by Abram Kardiner *et al.*, 394
HEDGPETH, J. W., [Oceanography] 325
HERSKOVITS, M. J., *The Dynamics of Culture Change*, by Bronislaw Malinowski, 237; *The Governing of Men*, by Alexander H. Leighton, 389
HERTZLER, A. E., *The Doctor's Job*, by C. Binger, 79

JANSSEN, R. E., *The Romance of Existence*, by Ross Bundy, 241

KROGMAN, W. M., *Mainsprings of Civilization*, by Ellsworth Huntington, 382

LEAKE, C. D., *The Autobiography of Science*, edited by Forest Ray Moulton and Justus J. Schifferes, 489

MARKS, L. S., *The Coming Age of Rocket Power*, by G. Edward Pendray, 240
MATHER, K. F., *Freedom Is More Than a Word*, by Marshall Field, 493
MEYERHOFF, H. A., and E. W. OLMSTEAD, *Principles of Physical Geology*, by Arthur Holmes, 327
MILLIKAN, R. A., *What Are Cosmic Rays?* by Pierre Auger, 157
MONTAGU, M. F. A., *The Psychology of Women, Vol. II: Motherhood*, by Helene Deutsch, 162; *The Biological Basis of Individuality*, by Leo Loeb, 491
MOULTON, E. J., *How to Solve It*, by George Polya, 321
MYERS, W. G., *The Story of Penicillin*, by Boris Sokoloff, 80

OEHSER, P. H., *South America Called Them*, by Victor Wolfgang von Hagen, 159; *Science Today and Tomorrow: 2nd Series*, by Waldemar Kaempffert, 323; *Science Year Book of 1945*, edited by John D. Ratcliff, 323; *Careers in Science*, by Philip Pollack, 323
OLMSTEAD, E. W., see MEYERHOFF, H. A.
OPLER, M. E., *The Social Systems of American Ethnic Groups*, by W. Lloyd Warner and Leo Srole 392

PENNELL, F. W., *Plants and Plant Science in Latin America*, edited by Frans Verdoorn, 319

SCHMIDT, K. P., *Mammals of the Pacific World*, by T. D. Carter, J. E. Hill, and G. H. H. Tate, 496
SINCLAIR, GEORGE, *An Introduction to Electronics*, by Ralph G. Hudson, 158
SINCLAIR, J. G., *Poet Physicians*, by Mary Lou McDonough, 160
STIEGLITZ, E. J., *New Goals for Old Age*, edited by George Lawton, 492; *You Are Younger Than You Think*, by Martin Gumpert, 492; *Ageing and Degenerative Diseases*, Vol. XI, Biological Symposia, by various authors, 492

TUKEY, H. B., *Thomas Jefferson's Garden Book*, annotated by Edwin M. Betts, 77

Original Verse

DANA, F. C., Evolution, 306

MORRILL, T. A., Our Home, 464

OEHSER, P. H., Book Reviewer (r.i.p.), 162; The Owls of the Brownstone Tower, 504

PIETENPOL, W. B., To a Realm Unreal, 100

SCHMIDT, K. P., Energy, 172; Entropy, 172
SINCLAIR, J. G., Theca, 352; Bird's-Eye View, 412

WYLIE, C. R., JR., Uncertainty, 371; Apologia, 426

Comments and Criticisms

BORING, E. G., Prejudice against Prejudice, 499

COOK, S. R., Electronics, 163

DAVIS, A. L., On Leake's "Ethicogenesis," 330

ELLIOTT, LOUIS, Prometric, 419

FLYNN, J. E., Antimetric, 419

GRANT, CHAPMAN, "When the Comet Struck America" vs. "Neptune's Racetracks," 243
GUDGER, E. W., Our Dear Watson, 163

HINKLE, L. E., JR., Dear 'Fessor, 81
HIXON, H. W., Beachcombing, 502
HO HUM, Melly Clismas, 502

JONES, BASSETT, Beachcombing, 502
KOONCE, S. D., Proselyte or Temporize? 82

MATHER, K. F., Malthus Rides Again, 83

NACHOD, F. C., Ethical Dualism, 499

OLD HI HO, Melly Clismas, 502

O'NEILL, A. M., Reactions to Leake's "Ethicogenesis," 330

ROSENBERG, ANNE, Reactions to Leake's "Ethicogenesis," 329

SELIGSBERGER, L., Reactions to Leake's Ethicogenesis," 329
SHEPARD, A. O., The Scientific Spirit and Human Prejudice, 497
SOPER, H. W., Soporific, 163

WELTFISH, GENE, The Last Word, 500
WOODCOCK, H. E., Some Insect Infants, 163 (light verse)

SUBJECT INDEX

Principal Articles

Aeronautics (J. F. Victory) 227; (S. R. M. Reynolds) 277
Age (F. B. Sumner) 143
Age factor in stellar performance (H. C. Lehman) 127
Agriculture (C. J. Brand and H. R. Smalley) 7; (E. B. Shaw) 101
Alcoholism (Carney Landis and Jane Cushman) 469
Amazon, The (F. F. Netto) 33, 90
Amchitka (Paul Guggenheim) 21
Anthropology (Paul Guggenheim) 21
Archeology (Paul Guggenheim) 21
Argentine beef (E. B. Shaw) 101

Beach cusps (O. F. Evans) 245
Beef, Restrictions on (E. B. Shaw) 101
Bimini (E. A. Andrews, R. P. Bigelow, T. H. Morgan) 333
Botany (F. G. Speck and E. S. Dodge) 63; (W. C. Steere) 114; (S. A. Cain) 421
Boulder dam (R. M. Glendinning) 181
Brazil (F. F. Netto) 33, 90; (Norman Bekkedahl) 199
Bread, Britain's National (Harriette Chick and Alice Copping) 226
Britain (William Laas) 109; (Harriette Chick and Alice Copping) 226

Carolina bays (Chapman Grant) 443
Cartography (S. W. Boggs) 45; (William Laas) 109; (A. L. Shalowitz) 249
Characteristics of science (Leo Kartman) 67; (P. H. Oehser) 378; (J. D. Bernal) 474
Chinchon, Countess of (G. Urdang) 17
Cinchona (G. Urdang) 17; (W. C. Steere) 114
Cockroaches (J. A. G. Rehn), 265
Color perception in insects (H. B. Weiss) 51
Communication, Electrical (John Mills) 138
Compulsory military service (A. G. Keller) 85; (T. H. Hazlehurst) 289
Control of disease (R. A. Moore) 173
Crop feeding (C. J. Brand and H. R. Smalley) 7

Death (F. B. Sumner) 143
Diet (Samuel Brody) 213
Disease (R. A. Moore) 173

Education (A. G. Keller) 85; (Max Black) 165; (T. H. Hazlehurst) 289; (A. J. Carlson) 379
Electricity (John Mills) 138
Entomology (H. B. Weiss) 51; (J. A. G. Rehn) 265
Ethnology (F. G. Speck and E. S. Dodge) 63; (H. L. Shapiro) 345

Fertilizers (C. J. Brand and H. R. Smalley) 7
Foot-and-Mouth disease (E. B. Shaw) 101
Forbes, Edward (J. W. Hedgpeth) 194
Fuel requirements (S. S. Visher) 313

Genetics (Leo Kartman) 67
Geography (S. W. Boggs) 45; (William Laas) 109; (R. M. Glendenning) 181; (A. L. Shalowitz) 249
Geology (O. F. Evans) 245; (Chapman Grant) 443
Gosse, Philip H. and Edmund (J. W. Hedgpeth) 194
Grapes (J. L. Fennell) 465

Hormones (Harry Benjamin) 427

Ichthyology (E. W. Gudger) 451; (Chapman Grant) 443
India (R. W. Phillips) 397

Malnutrition (Samuel Brody) 213
Marine zoology (J. W. Hedgpeth) 194; (E. A. Andrews, R. P. Bigelow, and T. H. Morgan) 333
Medicine (F. G. Speck and E. S. Dodge) 63; (R. A. Moore) 173
Mental disease (Carney Landis and Jane Cushman) 469
Meteorology (S. S. Visher) 313
Metric system (Oystein Ore) 372

NACA (J. F. Victory) 227
National Roster of Scientific and Specialized Personnel (R. C. Smith) 287, 359
Natural history (E. H. Walker) 307; (A. F. Skutch) 361
Navigation (A. L. Shalowitz) 249
Nutrition (Samuel Brody) 213; (Harriette Chick and Alice Copping) 226

Oath, the scientist's (Gene Weltfish) 210
Ornithology (A. F. Skutch) 361

Peace (A. E. Parr) 5
Performance, intellectual and physical (H. C. Lehman) 127
Pharmacology (G. Urdang) 17; (F. G. Speck and E. S. Dodge) 63; (W. C. Steere) 114
Philosophy (F. B. Sumner) 143; (Max Black) 165; (Patrick Romanell) 293; (Paul Crissman) 455
Physiology (E. F. Adolph) 57; (S. R. M. Reynolds) 277; (Harry Benjamin) 427
Postwar world (A. E. Parr) 5
Prohibition (Carney Landis and Jane Cushman) 469
Psychology (H. C. Lehman) 127

Quinine (G. Urdang) 17; (W. C. Steere) 114

Religion (F. B. Sumner) 143; (D. M. Myers) 150
Rubber (Norman Bekkedahl) 199

Social science (A. G. Keller) 85; (Joseph Schneider) 353; (J. D. Bernal) 474
Steinach, Eugen (Harry Benjamin) 427

Transport (S. W. Boggs) 45

Vitamins (Samuel Brody) 213

World calendar (Oystein Ore) 372

Science on the March

Aerosol bombs (L. D. Goodhue) 413
Agriculture (K. S. Chester) 483
American universities in Europe (W. A. Albrecht) 488
Atomic bomb (F. L. Campbell) 233
Award, 1945 John Scott (L. D. Goodhue) 413

Berlin conference (H. A. Meyerhoff) 317
Biarritz American University (W. A. Albrecht) 488

Chinese physiological research (W. H. Adolph) 153
Chlorophyll stain (M. V. Bailey) 234
Cotton defoliation (M. V. Bailey) 234
Cuckoos, Nesting habits of (D. E. Davis) 72

Defoliation (M. V. Bailey) 234

Foliage control (M. V. Bailey) 234

Green leafy vegetables (S. H. Wittwer) 71

Hematology (P. P. H. De Bruyn) 485

Insecticides (L. D. Goodhue) 413

Kofoid, Charles Atwood (H. Kirby) 415

McFadden, Edgar (K. S. Chester) 483

Nesting habits of cuckoos (D. E. Davis) 72
Nutrition (S. W. Wittwer) 71; (W. H. Adolph) 152

Physiological research in China (W. H. Adolph) 153
Plant pathology (K. S. Chester) 483
Postwar world (F. L. Campbell) 233; (H. A. Meyerhoff) 317

Russian research in physics (A. Jaffe) 154

Science at the Berlin Conference (H. A. Meyerhoff) 317
Sullivan, Capt. W. N. (L. D. Goodhue) 413

Tsing Hua University Physiological Laboratory (W. H. Adolph) 153

University of California Zoology Department (H. Kirby) 415

White blood cells, Locomotion of (P. P. H. De Bruyn) 485

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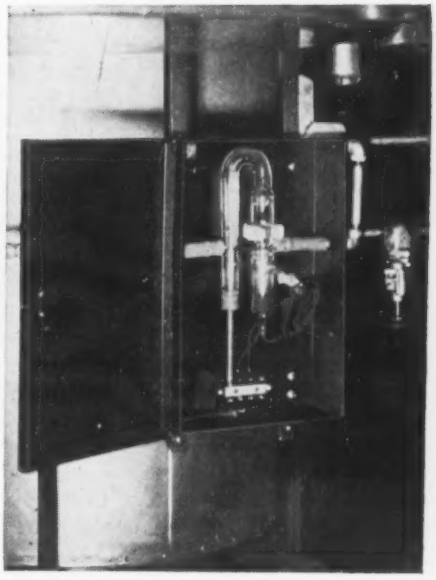
One of the equipment's greatest advantages, in a large laboratory, is that the automatically-drawn record chart gives written proof of water purity.

Catalog EN-95 describes the equipment and will be sent on request, but if you have a definite application in mind, an L&N engineer will be glad to supply individualized information.



Jrl Ad N-95(2)

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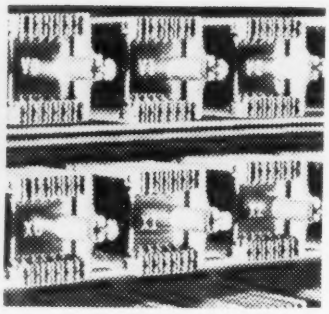
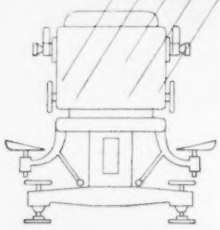
LEEDS & NORTHRUP

MEASURING INSTRUMENTS · TELEMETERS · AUTOMATIC CONTROLS · HEAT-TREATING FURNACES

Target practice with Relays and Keys





(Left to right) The operator punches the problem data on tape, which is fed into the computer. The solution emerges in the teletype receiver. Relays which figure out the problem look like your dial telephone system.

In designing the gun-control systems which shot down enemy planes, Army ballistic experts were faced by long hours of mathematical calculations.

So Bell Laboratories developed an electrical relay computer. It solved complicated problems more accurately and swiftly than 40 calculators working in shifts around the clock.

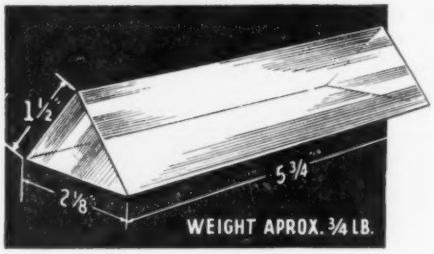
Resembling your dial telephone system, which seeks out and calls a telephone number, this brain-like machine selects and energizes electric circuits to correspond with the numbers fed in. Then it juggles the circuits through scores of combinations. It will even solve triangles and consult mathematical tables. The operator hands it a series of problems with the tips of her fingers —next morning the correct answers are neatly typed. Ballistic experts used this calculator to compute the performance of experimental gun directors.

In battle action, Electrical Gun Directors are instantaneous. Such a director helped to make Antwerp available to our advancing troops by directing the guns which shot down more than 90% of the thousands of buzz bombs.

Every day, your Bell System telephone calls are speeded by calculators which use electric currents to do sums. Lessons learned are being applied to the extension of dialing over toll lines.

BELL TELEPHONE LABORATORIES EXPLORING AND INVENTING, DEVISING AND PERFECTING FOR CONTINUED IMPROVEMENTS AND ECONOMIES IN TELEPHONE SERVICE

BARGAINS in WAR SURPLUS LENSES & PRISMS



ALL ITEMS FINELY GROUND and polished but edges slightly chipped or other slight imperfections which we guarantee will not interfere with their use. Come neatly packed and marked. Excellent for **XMAS** gifts.

TO KEEP POSTED on all our new Optical Items, send 10¢ and your name and address to get on our regular "Flash" mailing list.

TANK PRISMS

In order that the tank driver shall not get shot in the face, 2 of these Silvered Prisms are used to make a Periscope. We have secured a number of these, 90-45-45 degree Prisms of huge size—5¾" long, 2⅛" wide, finely ground and polished. Used to build a Periscope, camera stereo attachment, range finder, etc. Excellent also for experiments, classroom demonstrations. Prism easily converted into desk name plate by affixing gold letters. 100 gold letters supplied at only 10¢. (Order **Stock #3008-X**). Normally these Prisms would retail from $24 to $30 each.

SILVERED TANK PRISM
Stock #3004-X ..... **$2.00 each Postpaid**

PLAIN TANK PRISM
Stock #3005-X ..... **$2.00 each Postpaid**

FOUR TANK PRISMS—Special—**$7.00 Postpaid** . . . The most sensational bargain we have ever been able to offer.

TO SEE THE COLORS OF THE SPECTRUM, hold a plain tank prism in sun's rays. White incident light which passes through prism is thus broken up into a band of primary colors, the spectrum—a beautiful sight! **CLEANING BRUSH SET**—12" flexible plastic handle, hollow circular construction. 4 brushes to set, range stiff to soft. **Stock #504-X** (Reg. $6.00 value) **Price $1.00**

RAW OPTICAL GLASS—An exceptional opportunity to secure a large variety of optical pieces, both Crown and Flint glass (seconds) in varying stages of processing. Many prism blanks.
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RETICLE SET—5 assorted, engraved reticles from U. S. Gunsights.
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SPECTROSCOPE SETS . . . These sets contain all Lenses and Prisms you need to make a Spectroscope plus **FREE** 15-page Instruction Booklet.
Stock #1500-X—Hand Type Spectroscope, **$3.45 Postpaid**
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3049-X	Right Angle	69 mms.	167 mms.	10.00
3047-X	Right Angle	53 mms.	103 mms.	4.00
3038-X	Roof Prism	18 mms.	34 mms.	2.50
3042-X	Right Angle	41 mms.	40 mms.	1.00
3045-X	Right Angle	70 mms.	168 mms.	8.00
3001-X	Lens Surface	20 mms.	14 mms.	2.00
3006-X	Porro-Abbe	9 mms.	9 mms.	.25
3009-X	Porro	52 mms.	25 mms.	1.00
3010-X	Porro	43 mms.	21 mms.	.50
3016-X	Pentagon	45 mms.	22 mms.	.75
3029-X	Dove	16 mms.	65 mms.	1.25
3036-X	80 Degree Roof	60 mms.	36 mms.	4.00
6136-X	Rhomboid	20 mms.	17 mms.	.50

ALL THE LENSES YOU NEED TO MAKE YOUR OWN TELESCOPE!

All Are Achromatic Lenses

GALILEAN TYPE—Simplest to Make but has Narrow Field of View.
Stock #5018-X—4 Power Telescope ..... **$1.25 Postpaid**
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TERRESTRIAL TYPE—Have Much Wider Field of View than Galilean Type.
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PRISM TELESCOPES—Uses Prism instead of Lenses to Erect Image and are much shorter than Terrestrial Type. Have wide field of view.
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Stock #5011-X—11 Power Telescope ..... **$3.25 Postpaid**
Stock #5012-X—20 Power Telescope ..... **$7.25 Postpaid**

MICROSCOPE SETS

Consisting of 4 Cemented Achromatic Lenses for making a 40 Power Pocket Microscope or 140 Power regular size Microscope. These color corrected Lenses will give you excellent definition and may be used for micro-photography.
Stock #1027-X ..... **$3.00 Postpaid**
Consisting of Prism, Mirror and Condensing Lense. These used together with Stock #1037-X will make an excellent Microprojector enabling you to get screen magnification of 400 to 1000 Power according to screen distance.
Stock #1038-X ..... **$2.00 Postpaid**

14-POWER COLOR CORRECTED MAGNIFIER SET—Consists of 2 perfect 18 mm. diam. Achromatic Lenses and section of metal tubing for mount.
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LENS FOR KODACHROME EYE-VIEWER—Color corrected cemented Lens 38 mm. diam., 2 inch F.L.
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OPTICS FROM 4-POWER PANORAMIC TELESCOPE—Excellent condition. Consists of Objective Prism, Dove Prism, Achromatic Objective Lens, Amici Roof Prism, Eye Lens Set (. . . a $60.00 value).
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MONOCULAR SET OF LENSES AND PRISMS . . . From Navy's 7 x 50 Binocular. All the optics you need to make a 7 Power Monocular. (Buy 2 sets to make a Binocular.) Complete Directions included.
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THICK FIRST SURFACE MIRROR—From Rangefinder—size 78 x 94 mms.—½" thick very flat optical glass. May have very slight scratch on chromium and aluminum coated surface. A $60.00 value.
Stock #553-X ..... **$2.00 Postpaid**

TANK PERISCOPE—Complete Set Mounted Components—Perfect condition, originally constructed for U. S. Tank Corps. Consists of 2 fine Periscope Mirrors mounted in metal and plastic. Only plywood body frame is required to finish this exceptional Periscope. First surface mirror is well protected by glass windows. Set weighs 2⅔ lbs. Overall length of mount 6⅛", width 2⅛". Would normally retail at $40 to $50.
Stock #700-X ..... **$3.00 Complete Set Postpaid**
TWO SETS (4 UNITS) .... SPECIAL $5.50 POSTPAID

Order by Set or Stock No.—Satisfaction Guaranteed—Immediate Delivery

EDMUND SALVAGE COMPANY, P. O. Audubon, N. J.

All in favor raise right hands . . . *with wallets*

Naturally we want our boys home. The sooner the better.

But how much are we willing to do about it?

Are we willing to pay for bringing them back? If we are, we'll buy *extra* Bonds in the Victory Loan.

And after these fellows get home—these men who have fought and won the toughest war America has ever known—what then?

We want to take care of the injured ones, of course. We want to see that the young fellows who went off to fight get a chance to finish their education. We want to see that there are jobs—plenty of decent jobs—for the men who've been doing the world's meanest job at army pay.

How much are we willing to help?

If we're really serious about wanting our men to get what they have so richly earned, we'll buy *extra* Bonds in the Victory Loan.

Now's the time. Let's have a show of hands—with wallets—to prove how much we really want to hear that old familiar step and that familiar voice yelling "It's *me!*" Let's prove, with pocketbooks, that we can do our job as well as they did theirs.

THEY FINISHED THEIR JOB—
LET'S FINISH OURS!

VICTORY LOAN

American Association for the Advancement of Science

Smithsonian Institution Building
Washington 25, D. C.

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The SCIENTIFIC MONTHLY

December 1945

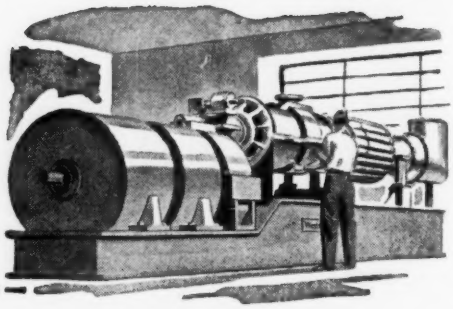
CONTENTS

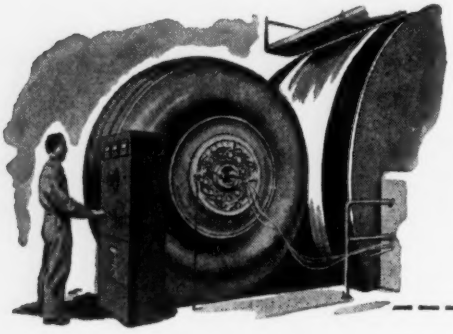
Pic la Rhune . . . . . *Stanley A. Cain* 421
Apologia (Verse) . . . . . *Clarence R. Wylie, Jr.* 426
Eugen Steinach, 1861–1944: A Life of Research . . . *Harry Benjamin* 427
A Biological Explanation of the Carolina Bays . . . *Chapman Grant* 443
Is the Giant Catfish, *Silurus glanis*, a Predator on Man? . *E. W. Gudger* 451
Causation, Chance, Determinism, and Freedom in Nature . *Paul Crissman* 455
The Tropical Grape . . . . . *Joseph L. Fennell* 465
Prohibition and Alcoholic Psychoses . *Carney Landis & Jane Cushman* 469
Transformation in Science . . . . . *J. D. Bernal* 474
Freedom of Research . . . . . *F. W. Preston* 477
Science on the March 483
Book Reviews 489
Comments and Criticisms 497
Index 505
Meet the Authors vii

PUBLISHED BY THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE
Smithsonian Institution Building, Washington 25, D. C.

In a laboratory a **SCIENTIST** experiments with a new gas turbine—using heat-resisting alloy blades that are far stronger, at 1100°F., than *ordinary* steel at room temperature.

... the name on the GAS TURBINE is Westinghouse.





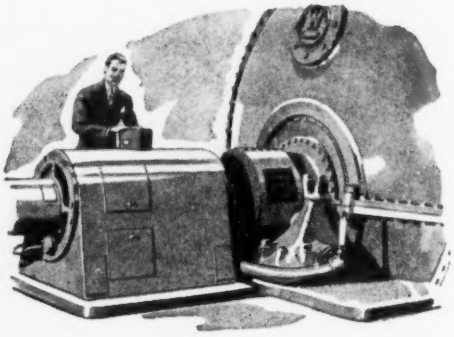
On a special machine a **TESTER** employs a Rototrol* for smoothly accelerating a large flywheel, used in determining the wear-resisting qualities of tires and brakes—for huge air transports of the future.

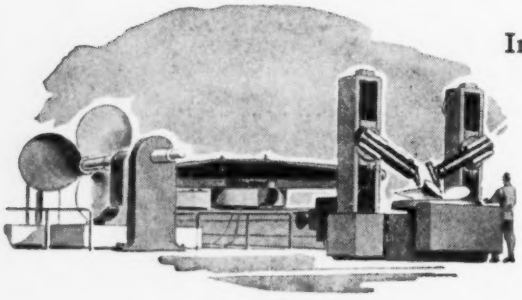
... the name on the ROTOTROL is Westinghouse.

*Registered Trademark

In a power plant an **ENGINEER** uses a Vibrograph to "take the pulse" of a turbo-generator . . . recording the smallest vibrations as a trace on a film.

... the name on the VIBROGRAPH is Westinghouse.





In a manufacturing plant an **OPERATOR** uses an electronic control to regulate the movement of milling cutters—for accurately machining irregular contours on giant ship propellers.

... the name on the ELECTRONIC CONTROL is Westinghouse.

Westinghouse

PLANTS IN 25 CITIES OFFICES EVERYWHERE

NOW THAT Westinghouse technical skill and "know-how" have turned from war to peace, expect great things . . . from Westinghouse research, engineering, and precision manufacture.

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THE *Eastman Spectroscopic Plate, Type 1-Z,* has been improved in order to give it approximately five times the sensitivity of the previous plate at about λ10,800Å. It also has somewhat finer granularity. The increased sensitivity is obtainable only with correct hypersensitizing. The improved plate offers a valuable aid to spectrography out to the limit to which it is now possible to photograph.

More than 100 kinds of Eastman Spectroscopic Plates are available. Detailed information on their properties can be found in the booklet, *Photographic Plates for Use in Spectroscopy and Astronomy* (*Fifth Edition*), a copy of which will be sent free on request.

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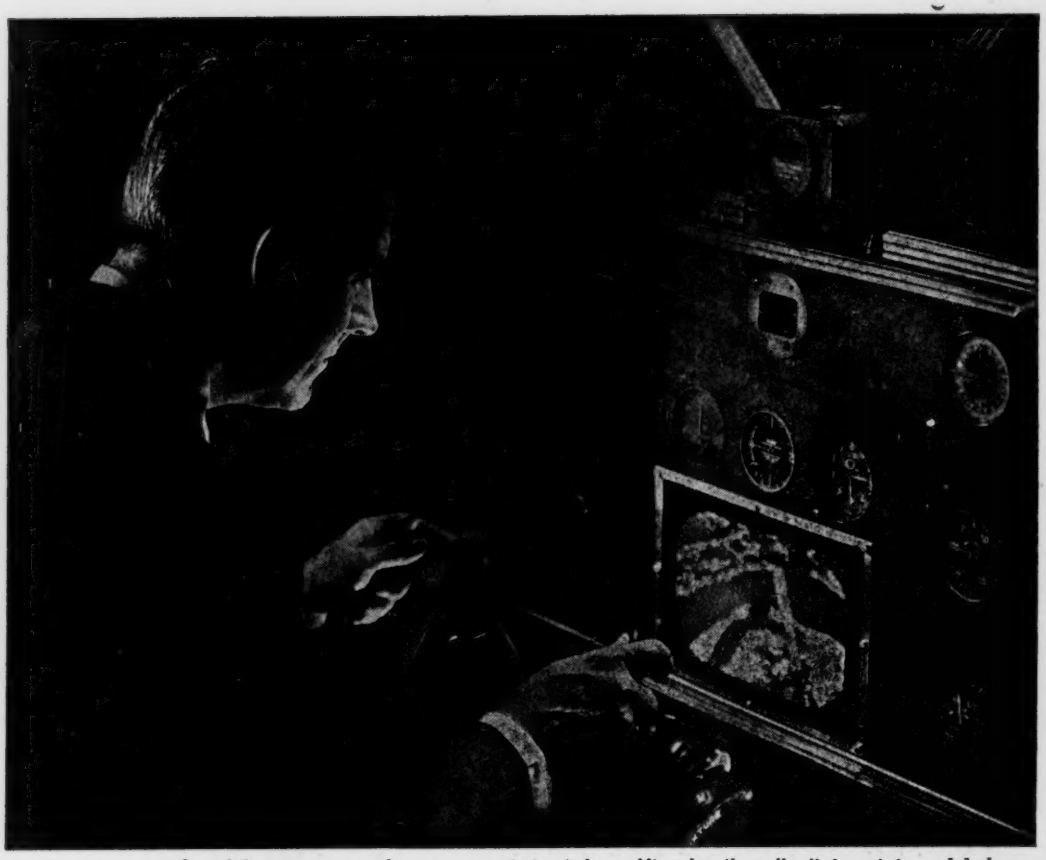
Research Laboratories **Rochester 4, N. Y.**



One of the many ways radar can serve aviation is by enabling the pilot to "see" through fog and darkness.

Radar – a "Moving Roadmap" for Flying

Pilots can now have an accurate radar "roadmap" of the earth below—showing landmarks and major details of terrain. Radar will make it a whole lot safer to fly at night or in stormy weather—as well as in broad daylight.

This is only one of the many possible uses for radar. For example, radar will "see" icebergs or islands many miles away—day or night—and will thus enable ships to avoid them. It will provide men with an amazing new "sixth sense"—and will be used in a great many ways yet to be discovered.

RCA research and engineering played a leading role in developing radar . . . the same research that goes into *all* RCA products.

And when you buy an RCA Victor radio or television set or Victrola, made exclusively by RCA Victor, you enjoy a unique pride of ownership. For you know, if it's an RCA it is one of the finest instruments of its kind that science has achieved.

Radio Corporation of America, Radio City, New York 20. *Listen to The RCA Show, Sundays, at 4:30 P.M., E.T., over the NBC Network.*

HOW RADAR WAS BORN

During RCA experiments at Sandy Hook in the early 1930's, a radio beam was shot out to sea. Men listening with earphones discovered that this beam produced a tone upon hitting a ship that was coming into the New York harbor.

Later on the question arose, "If radar could 'hear' couldn't it be made to 'see'?" So the viewing screen—or scope—was incorporated into radar. This scope is an outgrowth of all-electronic television that was invented and perfected at RCA Laboratories.

RCA **RADIO CORPORATION of AMERICA**